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Abstract

Evidence from behavioral as well as neurophysiological studies indicates the occurrence of active task-order coordination processes in dual-task situations. These processes are required for planning and regulating the processing sequence of two tasks that overlap in time. So far, however, the cognitive and neural mechanisms underlying active task-order coordination are highly underspecified. To tackle this issue, in the present dissertation I tested a model of task-order coordination in dual-task situations. This model assumes that task-order coordination relies on representations that contain information about the processing sequence of the two component tasks. In addition, the model includes assumptions about the (1) locus of processing as well as (2) the exact content of these order representations. The model further assumes that (3) the lateral prefrontal cortex is causally involved in implementing task-order coordination processes and that (4) these processes are affected by different order criteria.

I tested this model in a series of four studies by applying a dual-task paradigm with randomly changing task order. I demonstrated that task-order representations are actively maintained and processed in working memory during dual tasking. Moreover, I found that these order representations only contain information about the processing sequence of tasks, whereas specific component task information is represented separately. By applying transcranial magnetic stimulation, I also provided evidence for the causal role of the lateral prefrontal cortex for task-order coordination. Furthermore, I showed that the demands on task-order coordination are increased when participants have to adhere to an external and mandatory order criterion compared to when they can use an internally generated order criterion that is based on free choice. The implications of these results as well as an outlook for future research will be discussed in the framework of the proposed model.

keywords: dual-task situations, PRP paradigm, central bottleneck, task-order coordination, executive control, transcranial magnetic stimulation, lateral prefrontal cortex

Zusammenfassung

Ergebnisse aus behavioralen und neurophysiologischen Studien liefern Hinweise für das Auftreten von aktiven Prozessen der Reihenfolgekoordination in Doppelaufgabensituationen. Diese Prozesse sind notwendig für die Planung und Regulation der Bearbeitungsreihenfolge von zwei zeitlich überlappenden Aufgaben. Bisher ist jedoch wenig über die kognitiven und neuronalen Mechanismen bekannt, die aktiver Reihenfolgekoordination zugrunde liegen. Ziel der vorliegenden Dissertation war deshalb die Überprüfung eines Modells aktiver Prozesse der Reihenfolgekoordination in Doppelaufgabensituationen. Das Modell nimmt an, dass diese Prozesse auf Repräsentationen zurückgreifen, die Informationen über die Verarbeitungssequenz zweier Aufgaben enthält. Zusätzlich macht das Modell Annahmen über (1) den Ort der Verarbeitung und (2) den genauen Inhalt dieser Reihenfolgerepräsentationen. Weiterhin enthält das Modell die Annahmen, dass (3) der laterale präfrontale Kortex kausal in die Implementierung von Reihenfolgekoordinationsprozessen involviert ist und dass (4) diese Prozesse von unterschiedlichen Kriterien bezüglich der Reihenfolge beeinflusst werden.

In der vorliegenden Dissertation wurde dieses Model in einer Reihe von vier Studien überprüft. Dazu wurde ein Doppelaufgabenparadigma mit zufällig wechselnder Aufgabenreihenfolge verwendet. Ich konnte zeigen, dass die notwendigen Reihenfolgerepräsentationen während der Bearbeitung von Doppelaufgaben im Arbeitsgedächtnis aufrechterhalten und aktiv verarbeitet werden. Ich konnte weiterhin zeigen, dass diese Reihenfolgerepräsentationen nur Information über die Sequenz der Aufgabenverarbeitung enthalten. Spezifische Information bezüglich der Komponentenaufgaben wird hingegen separat repräsentiert. Durch den Einsatz transkranieller Magnetstimulation konnte ich zudem eindeutige Hinweise dafür liefern, dass der laterale präfrontale Kortex eine wichtige kausale Rolle für Reihenfolgekoordination spielt. Darüber hinaus konnte ich zeigen, dass Anforderungen an Reihenfolgekoordinationsprozesse in Situationen, in denen Probanden ein von außen vorgegebenes Reihenfolgekriterium befolgen, erhöht sind im Vergleich zu Situationen, in denen Probanden ein selbstgeniertes und auf einer freien Wahl basierendes Kriterium nutzen können. Die Implikationen dieser Ergebnisse und Vorschläge für zukünftige Forschung werden unter Berücksichtigung des vorgeschlagenen Modells diskutiert.

Schlüsselwörter: Doppelaufgaben, PRP-Paradigma, zentraler Flaschenhals, Reihenfolgen-koordination, exekutive Kontrolle, transkranielle Magnetstimulation, präfrontaler Kortex

List of original research articles

This dissertation is based on four original research articles:

Article 1

Kübler, S., Strobach, T., & Schubert, T. (1^{st} revision in preparation). The role of working memory for task-order coordination in dual-task situations. Submitted to *Psychological Research*.

Article 2

Kübler, S., Strobach, T., & Schubert, T. (1^{st} revision in preparation). On the organization of order and task specific information in dual-task situations. Submitted to *Journal of Experimental Psychology: Human Perception and Performance*.

Article 3

Kübler, S., Soutschek, A., Schubert, T. (2019). The causal role of the lateral prefrontal cortex for task-order coordination in dual-task situations: A study with transcranial magnetic stimulation. *Journal of Cognitive Neuroscience*, *31*(12), 1840-1856. doi:10.1162/jocn_a_01466

Article 4

Kübler, S., Reimer, C.B., Strobach, T., & Schubert, T. (2018). The impact of free-order and sequential-order instructions on task-order regulation in dual tasks. *Psychological Research*, *82*, 40-53. doi:10.1007/s00426-017-0910-6

1. Introduction

Multitasking, i.e. performing more than one task at the same time, often comes at a cost compared to performing individual tasks in isolation. For example, people may be less focused during a meeting when they also respond to e-mails on their mobile phones, or they may produce slower breaking responses during car driving when chatting with a front-seat passenger.

In the last decades, scientific interest for multitasking has been growing and related phenomena have been investigated in the laboratory by employing different approaches, such as dual-task paradigms with temporally overlapping component tasks (Fischer & Plessow, 2015; Koch, Poljac, Müller, & Kiesel, 2018; Schubert, 2008). A typical observation in these paradigms is that performance is usually impaired in dual-task compared with single-task situations (Neider et al., 2011; Strobach, Salminen, Karbach, & Schubert, 2014). These dual-task costs have been attributed to a capacity limitation in central attention (Pashler, 1994; Schubert, 1999). According to many theories, this limitation constitutes a bottleneck during dual-task processing which results in the serial processing of two tasks that overlap in time.

Within the last thirty years, research has mainly addressed questions concerning the exact locus or nature of this central bottleneck (e.g. Logan & Gordon, 2001; Meyer & Kieras, 1997; Pashler & Johnston, 1989) and whether or not interventions such as dual-task practice can assist in overcoming the central bottleneck resulting in reduced dual-task costs and, thus, improved multitasking performance (e.g. Schumacher et al., 2001; Strobach, Frensch, & Schubert, 2012). However, an equally important but far more neglected issue concerns the cognitive challenges that arise due to bottleneck processing (Schubert, 2008). More specifically, since both tasks are processed serially at the bottleneck stage, there is the requirement to schedule the processing order of both tasks and regulate which task is processed first and which task is processed second (Sigman & Dehaene, 2006; Szameitat, Schubert, Muller, & Von Cramon, 2002).

Early bottleneck accounts implied a rather passive and inflexible "first-come first-served" mechanism for regulating task order (De Jong, 1995; Strobach, Hendrich, Kübler, Müller, & Schubert, 2018). According to this assumption, the order of task processing is simply determined by which of the two task processing streams arrives at the bottleneck first. Thus, regulating task order proceeds without the employment of additional control processes. In contrast, there is now a growing body of evidence from studies applying behavioral (Luria &

Meiran, 2003, 2006) and neuroscientific research methods (Stelzel, Kraft, Brandt, & Schubert, 2008; Strobach, Soutschek, Antonenko, Floel, & Schubert, 2015) indicating that additional task-order coordination processes, which rely on explicit order information, are required for regulating the processing order of tasks. However, these task-order coordination processes are still little understood and many questions remain open until today. How do these order coordination processes work? Where and how is task-order information stored and processed? Which neural structures are causally involved in implementing task-order coordination? Which role do instructions play for the recruitment of task-order coordination?

The aim of the present dissertation was to tackle these questions and to shed light on the underlying cognitive mechanisms and the neural implementation of task-order coordination processes. For this purpose, I tested a conclusive and detailed model of task-order coordination in dual-task situations. A core assumption of this model is that task order is regulated by executive control processes that act on explicit order representations. The model further assumes that these order representations are maintained and processed in working memory in addition to and separately from the representations of the component tasks. The model also hypothesizes that, on a neural level, the dorsolateral prefrontal cortex (dlPFC) plays a causal role for implementing task-order coordination processes. Additionally, the model predicts that the employment of task-order coordination processes depends on the relevant order criterion specified by task instructions.

1.1 Overview over the present work

In Chapter 2, I will introduce the psychological refractory period (PRP) paradigm and explain in more detail how the presumption of additional task-order coordination processes complements dual-task models that assume a capacity limitation in central attention. In Chapter 3, I will summarize previous work on active task-order coordination in dual-task situations, before I will outline the tested model of task-order coordination in Chapter 4. In Chapter 5, I will give an overview over the methods I applied in this dissertation and present the specific research questions. Studies 1 – 4 will then be summarized in the Chapters 6 – 9, respectively. In Chapter 10, I will further discuss the empirical findings and their implications for the current model and provide an outlook for future research.

2. Dual-Task situations and the central bottleneck

2.1 The psychological refractory period (PRP) paradigm

In the laboratory, dual-task performance can be investigated in a plethora of different paradigms. A prominent example is the PRP paradigm in which participants are asked to perform two speeded choice reaction time (RT) tasks, Task 1 and Task 2, with varying temporal overlap (Pashler & Johnston, 1989; Schubert, 1999; Telford, 1931; Welford, 1952). In the present dissertation, this PRP paradigm provides a useful framework for investigating task-order coordination processes for several reasons. First, as the PRP paradigm usually employs target stimuli with clearly defined on- and offsets, the order of stimulus presentation can be manipulated easily (see Chapter 3). Second, as the PRP paradigm requires discrete responses for both tasks, such as single key presses (in contrast to dual-task paradigms applying rather continuous tasks), inferences about the processing order at the bottleneck stage can be drawn. Capitalizing on these benefits, I applied a specific version of the PRP paradigm in Studies 1-4 in order to investigate task-order coordination processes in dual-task situations.

In the PRP paradigm, participants are asked to perform two choice RT tasks with varying temporal overlap. To this aim, the target stimuli of both tasks are presented one after the other with a variable temporal interval, i.e. the stimulus onset asynchrony (SOA), with the target stimulus of Task 1 presented first and the target stimulus of Task 2 presented second. Usually, participants are instructed to respond to both tasks as fast and accurately as possible first to Task 1 and then to Task 2. As a typical finding, short SOAs usually result in prolonged RTs for Task 2 (RT 2) compared with long SOAs (Pashler, 1994; Schubert, 1999). This increase in RT 2 from long to short SOA is referred to as the PRP effect. The RT for Task 1 (RT 1) is usually not affected by the SOA manipulation (Strobach, Schütz, & Schubert, 2015).

One of the most popular and well-established theories explaining the PRP effect is the central bottleneck account (Dux, Ivanoff, Asplund, & Marois, 2006; Lien, Schweickert, & Proctor, 2003; Pashler, 1984; Pashler & Johnston, 1989; Schubert, 1999, 2008). According to this account, while processing on the perception and motor stages can operate in parallel for both tasks, response selection is severely limited in capacity and, thus, only allows for the processing of one task at a time constituting a structural bottleneck. Importantly, this bottleneck assumption serves as a compelling explanation for the PRP effect. At short SOA, when temporal overlap between both tasks is high, Task 2 reaches the bottleneck shortly after

Task 1. Since response selection for Task 2 can only occur when response selection for Task 1 has finished, the processing of Task 2 is interrupted and postponed until Task 1 has left the bottleneck stage. At long SOA, response selection for Task 1 has already finished when Task 2 reaches the bottleneck. As a result, processing of Task 2 can proceed without any interruption. The additional waiting time for Task 2 at short SOA, thus, results in increasing RT 2 with decreasing SOA (Hein & Schubert, 2004; McCann & Johnston, 1992; Pashler, 1994).

Since its introduction, the central bottleneck account has been challenged by alternative models that contradict the rigid assumption of a strictly structural bottleneck. Prominent examples of these models are strategic bottleneck models (e.g. Logan & Gordon, 2001; Meyer & Kieras, 1997) and central capacity sharing models (e.g. Navon & Miller, 2002; Tombu & Jolicoeur, 2003). Without going into detail, these models also assume that response selection represents a processing stage with limited capacity. Nevertheless, parallel processing at this stage is in principle possible. However, many of these models suggest that serial response selection constitutes a default processing mode because it usually leads to optimized behavior in most multitasking situations (Fischer & Plessow, 2015; Koch et al., 2018). Parallel response selection, in contrast, seems to only occur under specific circumstances (e.g. Miller, Ulrich, & Rolke, 2009; Schumacher et al., 2001) or is only possible for a specific population of individuals (e.g. Brüning & Manzey, 2018; Watson & Strayer, 2010).

In the last 30 years, important findings have been reported that shed further light on bottleneck processing. Research questions that have been addressed so far concern for example the role of component task characteristics for bottleneck processing (e.g. Huestegge & Koch, 2010; Stelzel & Schubert, 2011), the potential modulation of bottleneck processing due to practice (e.g. Liepelt, Strobach, Frensch, & Schubert, 2011) and many more. However, a crucial question that has received far less attention concerns the scheduling of task processing at the bottleneck stage (Schubert, 2008). This is an important issue since a serially operating bottleneck raises the question of how task order is regulated in dual situations.

2.2 Bottleneck processing and task order

During dual-task situations the target stimuli are usually presented in quick succession which results in high temporal overlap between both component tasks. Since response selection usually proceeds serially, i.e. for one task at a time, the task processing streams of both tasks compete for access to the capacity-limited bottleneck at the response selection stage (Stelzel et al., 2008; Szameitat et al., 2002). As a result and in order to resolve this competition, the processing order of both tasks has to be determined and the central bottleneck has to be allocated to both tasks accordingly (Koch et al., 2018; Schubert, 2008; Strobach, Antonenko, et al., 2018). Classic response selection bottleneck accounts (e.g. Pashler, 1994) imply a rather passive mechanism for scheduling task order. According to this mechanism, the bottleneck is passively allocated to both task processing streams based on their central arrival times analogously to a “first-come, first-served” principle (De Jong, 1995; Hendrich et al., 2012). In other words, this “first-come, first-served” account proposes that the task that finishes perceptual processing first reaches the bottleneck first and simply enters the response selection stage. The other task, which arrives at the bottleneck second, is interrupted passively and has to wait until the other task has left the response selection stage before task processing can proceed. Thus, this passive queuing mechanisms does not require any additional processes for deliberately coordinating task order.

Although the assumption of a “first-come, first-served” principle has been tested and confirmed in recent years (Hendrich, Strobach, Buss, Mueller, & Schubert, 2012; Sigman & Dehaene, 2006; Strobach, Hendrich, et al., 2018) there is now a growing body of evidence indicating that, in addition to this passive queuing mechanism, active *task-order coordination processes* are required in dual-task situations (Luria & Meiran, 2003, 2006; Schubert, 2008; Steinhauser & Steinhauser, 2018; Szameitat et al., 2002). In contrast to the “first-come, first-served” mechanism, these task-order coordination processes deliberately regulate which task to process first and which task to process second and temporally coordinate both task processing streams along the central bottleneck. So far, however, only little is known about these active task-order coordination processes and many issues concerning their cognitive and neural underpinning remain unanswered. Does active task-order coordination rely on working memory? Which kind of information do task-order coordination processes rely on? Which brain regions are recruited for implementing task-order coordination? Is the degree to which task-order coordination processes are employed in dual-task situations dependent on the given task instructions? To answer these questions, in this dissertation I tested a model of task-order coordination in dual-task situations. Before I will present this model, I will first summarize and problematize the findings of previous studies on active task-order coordination and demonstrate in more detail which open questions need to be answered.

3. Active task-order coordination processes in dual-task situations

First evidence for the occurrence of active task-order coordination stems from studies that compared performance in dual-task situations with constant and random task order (e.g. Szameitat et al., 2002; Stelzel et al., 2008; Töllner, Strobach, Schubert, & Müller, 2012). For example, in the imaging study of Szameitat et al. (2002), participants performed a dual task consisting of an auditory (AUD) and a visual (VIS) component task. Both tasks had to be performed in quick succession separated by an SOA of +/-200 ms, with positive and negative values indicating that the auditory or visual stimulus was presented first, respectively. Thus, in this dual-task situation, either the auditory target was presented first and the visual target second (AUD→VIS trials) or vice versa (VIS→AUD trials). Participants were instructed to perform both tasks as fast and as accurately as possible. Importantly, they were also asked to respond to both tasks according to the order of stimulus presentation. That is, in AUD→VIS trials participants had to perform the auditory task first and then the visual task second, and in VIS→AUD trials participants had to perform the visual task first and then the auditory task second. In addition, the authors applied dual-task trials in either fixed-order blocks or random-order blocks. In fixed-order blocks, the SOA was not varied and the order of target stimuli remained constant (e.g. AUD→VIS – AUD→VIS – AUD→VIS – AUD→VIS – AUD→VIS, with the hyphen indicating a break between two consecutive trials). In random-order blocks, on the other hand, the SOA could either yield positive (i.e. 200 ms) or negative (i.e. – 200 ms) values. As a result, the order of target stimuli varied randomly from trial to trial (e.g. AUD→VIS – AUD→VIS – VIS→AUD – AUD→VIS – VIS→AUD). Consequently, in fixed-order blocks participants were able to employ the same task order throughout the entire block (e.g. they always performed the auditory task first and the visual task second). In random-order blocks, in contrast, participants had to adjust their processing order to the variable stimulus order due to the given instruction (e.g. on some trials they had to perform the auditory task first, on other trials the visual task).

When comparing participants' performance between both block types, the authors found increased RTs for both tasks in random-order blocks compared with fixed-order blocks. Furthermore, by applying functional magnetic resonance imaging (fMRI) the authors demonstrated that these performance decrements were accompanied by increased activation in posterior parts of the dlPFC, a region that has been consistently shown to be involved in the implementation of cognitive control processes (Brass, Derrfuss, Forstmann, & von Cramon, 2005; Brass, Liefooghe, Braem, & De Houwer, 2017; Derrfuss, Brass, Neumann, & von Cramon, 2005). According to the authors, these findings are not in line with the assumption of a "first-come, first-served" principle which assumes that that task order is exclusively determined by central arrival times and that no additional processes are needed for actively regulating the processing order of tasks. Instead, increased RTs and neural activation indicate the occurrence of active task-order coordination processes in random-order compared with fixed-order blocks. In particular, in fixed-order blocks, the control settings necessary for regulating task order can be prepared in advance before each block and there is no requirement for further adjustment during task performance. In random-order blocks, in contrast, the variable order of target stimuli requires the permanent change of control settings so that participants can adjust their processing order to the varying order of stimuli. As a result, during random-order blocks, participants have to employ task-order coordination processes in order to monitor the order of stimuli and match their processing order accordingly.

The occurrence of these task-order coordination processes in dual-task situations has been replicated in several studies and linked to additional neural processing in regions of the dlPFC (e.g. Stelzel et al., 2008; Strobach, Antonenko, et al., 2018; Strobach, Soutschek, et al., 2015). However, based on these studies, several issues remain unsolved. First, due to the nature of the fMRI method, studies employing this method can only provide correlational evidence for the association of a given brain region with a specific cognitive function (e.g. Logothetis, 2008). Consequently, even despite preliminary evidence from neuroimaging studies, it is not yet clear whether or not the dlPFC plays a causal role for implementing task-order coordination processes in dual-task situations.

Furthermore, and equally important, based exclusively on the results from studies comparing fixed-order and random-order blocks, the exact mechanisms underlying task-order coordination remain rather vague. According to more specific accounts, task-order coordination relies on explicit order information that is stored and actively processed in working memory (Luria & Meiran, 2003; Szameitat, Lepsien, von Cramon, Sterr, & Schubert, 2006). This account stems from studies that investigated task-order coordination processes in a more fine-grained fashion. In particular, rather than by comparing blocks with constant and variable task order, studies applying this research approach examined the occurrence of task-order coordination with higher temporal resolution on a trial-to-trial level (e.g. Szameitat, et al., 2006; see also De Jong, 1995; Luria & Meiran, 2003, 2006). For example, in the study of

Szameitat et al. (2006), the authors applied a similar dual-task situation as in their previous study (Szameitat et al., 2002). That is, participants also performed an AUD/VIS dual-task with variable stimulus order. Furthermore, participants were also instructed to perform both tasks according to the order of stimulus presentation. In contrast to the block comparison applied in their previous study, the authors investigated how changing order control settings relative to the preceding trial affects dual-task performance in the current trial. For this purpose, they distinguished two trial types: same-order trials and different-order trials. For same-order trials, the order of tasks in the current dual-task trial *n* is repeated compared to the previous trial *n-1*. For example, an AUD→VIS trial (trial *n*) is preceded by another AUD→VIS trial (trial *n-1*). For different-order trials, in contrast, the order of tasks in the current trial *n* is reversed compared to the previous trial *n-1*. For instance, an AUD→VIS trial (trial *n*) is preceded by a VIS→AUD trial (trial *n-1*). The authors demonstrated that performance was improved in same-order trials, which was reflected in faster RTs for both tasks compared with RTs in different-order trials. In addition to these behavioral differences, in different-order relative to same-order trials, the authors also found increased neural activation in the posterior dlPFC, close to the activation peaks found in earlier imaging studies investigating task-order coordination (Schubert & Szameitat, 2003; Szameitat et al., 2002).

According to Szameitat et al. (2006), the differences between same-order and different-order trials suggest that task-order coordination processes rely on an explicit order representation, i.e. the *task-order set* (see also Luria & Meiran, 2003; 2006; as well as Hirsch, Nolden, & Koch, 2017; Hirsch, Nolden, Philipp, & Koch, 2018). This task-order set specifies the processing order of both component tasks analogously to a to-do list. Due to the instruction to respond to the target stimuli as they appear, in each trial participants have to monitor the order of stimuli and activate the appropriate task-order set in working memory accordingly. This task-order set then schedules the processing of tasks by sequentially activating the task representations (i.e. task sets) of the component tasks. After its implementation in working memory, the task-order set remains active and, thus, affects performance in subsequent trials For the case of a same-order trial, when task order is repeated in the current trial *n* compared to trial *n – 1*, the task-order set of the previous trial can be re-applied. This is so because the task-order set of the previous trial specifies the same task order as in the current trial (e.g. AUD→VIS - AUD→VIS). For the case of a different-order trial, when task order is reversed in the current trial *n* compared to trial *n – 1*, the task-order set of the previous trial cannot be reemployed because it does not specify the correct task order of the current trial (e.g. AUD→VIS - VIS→AUD). Instead, participants have to overcome the order set of the previous trial and have to activate a new task order set in order to implement the appropriate task order and avoid a scheduling error. As a result, processing demands in different-order trials are increased compared to the processing demands in same-order trials. This is reflected in prolonged RTs and increased dlPFC activation.

Although the occurrence of task-order set based coordination processes in dual-task situations seems to be a reliable phenomenon (see for example De Jong, 1995; Steinhauser & Steinhauser, 2018; Strobach, Antonenko, et al., 2018; Strobach, Soutschek, et al., 2015), numerous questions remain open until today. One of these questions relates to the locus of processing of task-order information. Different authors assume that the task-order set is actively processed in working memory (e.g.Hirsch et al., 2017; Hirsch et al., 2018; Luria & Meiran, 2003, 2006; Szameitat et al., 2006). This perspective is also supported by recent accounts on working memory and its role for (dual-)task processing (Brass et al., 2017; Ellenbogen & Meiran, 2008; Oberauer, 2009). However, so far, it is still not clear whether the assumption that the task-order set is actively maintained and processed in working memory holds true since a direct test of this assumption is still pending. This is especially important, since the observation of performance differences between same-order relative and different-order trials can also be accounted for by alternative explanations. For example, performance benefits for same-order trials could simply indicate automatic priming of order information in long-term memory (Logan, 1988, 2002; Hommel & Eglau, 2002; Mayr & Bryck, 2005; Schneider & Logan, 2005) rather than its active processing in working memory.

An additional open question concerns the content of the task-order set. This is an crucial issue, since, in addition to task-order information, also specific information about the component tasks has to be maintained in an accessible state during dual-task processing (Ellenbogen & Meiran, 2008). According to some authors, the task-order set only contains order information without further specifying the component tasks (Luria & Meiran, 2003, 2006; Stelzel et al., 2008). Specific component task information, in contrast, is stored separately by a different type of representation, i.e. the task sets of the component tasks. Importantly, the assumption of distinct representations for task-order and specific component task information is in line with other accounts suggesting that multitasking situations are represented by a loose agglomeration of independent informational components with each of this component specifying a particular type of information necessary for the task at hand (Hübner, Futterer, & Steinhauser, 2001; Meiran, Kessler, & Adi-Japha, 2008). On the other hand, as has been suggested by other work groups (e.g. Hirsch et al., 2018), it is also possible that the task-order set contains task-order as well as specific component task information. This would mean that the task-order set would represent both types of information in an integrated fashion. So far, these two assumptions have not been tested against each other and evidence for the former or the latter perspective on the content of the task-order set is still lacking.

As a further open issue, it is unclear how different order criteria affect task-order coordination processes. Most studies investigated task-order coordination processes that were triggered by an external and mandatory order criterion (Luria & Meiran, 2003; Szameitat et al., 2006). In particular, due to instruction, in these studies participants had to adjust their order of task processing to the variable order of stimulus presentation. It has been hypothesized, however, that introducing a less strict and internally generated order criteria, which relies on free order choices, might change and even reduce demands on task-order coordination (Strobach, Hendrich, et al., 2018; Strobach, Kübler, & Schubert, 2019). Evidence for this assumption also stems from the field of task switching. Some studies applying this paradigm demonstrated that allowing for free task choices and, thus, encouraging self-organized task scheduling can, in fact, improve performance compared to task switching situations with externally determined task scheduling (Arrington & Logan, 2005; Brüning, Reissland, & Manzey, 2020; Gollan, Kleinman, & Wierenga, 2014). Until now, however, it is an open question, whether task-order coordination processes can be modulated in a similar fashion in dual-task situations.

To sum up, the occurrence of task-order set based coordination processes in dual-task situations has been replicated in a handful of studies (see for example De Jong, 1995; Steinhauser & Steinhauser, 2018; Strobach, Antonenko, et al., 2018; Strobach, Soutschek, et al., 2015). However, although these studies suggest that the occurrence of these processes seem to be a reliable phenomenon, numerous questions remain open until today. To answer these questions in this dissertation, I tested a conclusive model of task-order coordination in dual-task situations. This model will be outlined in the next chapter.

4. A model of task-order coordination processes in dual-task situations

Evidence suggests that the active and deliberate sequencing of task processing at the bottleneck stage relies on task-order coordination processes acting on explicit order information stored in the task-order set. However, the exact mechanisms underlying such task-order coordination processes are still highly underspecified and many questions remain unsolved. To address this issue, I tested a model of task-order coordination in dual-task situations in a series of several experiments. This model is based on prior findings but also includes novel and testable assumptions about the (1) locus of task-order set processing, (2) about the exact content of the task-order set, (3) about the neural underpinnings of task-order coordination processes, as well as about (4) the influence of instructions and different order criteria on these processes. This model is illustrated in Figure 1.

As a prerequisite, the model assumes that dual-task situations are characterized by a bottleneck (being it structural or strategic) that usually results in the serial processing of two tasks that overlap in time and, thus, requires the regulation of task order at the bottleneck stage. In addition to the passive "first-come, first-served" principle, which determines task order based on central arrival times, the model further assumes that task order is also regulated by active task-order coordination processes. These active task-order coordination processes rely on task-order representations, which I will call task-order sets (Szameitat et al., 2006; see also Luria & Meiran, 2003, 2006). These task-order sets contain information about the processing sequence of the component tasks in analogy to a to-do list (e.g. "First Task A, second Task B") and schedule task processing during dual-task situations.

So far, the model is in line with the assumptions based on evidence from previous research. In addition, as a first novel assumption, the model proposes that the task-order set is maintained and actively processed in working memory in addition to the specific component task information. More specifically, performing a dual-task trial requires selecting the appropriate task-order set and then implementing it in working memory. From there it guides task order by sequentially instantiating the processing of the component tasks. The assumption of active processing of the task-order set in working memory is in agreement with recent theoretical accounts on working memory and its role for single-task and multitasking situations (Brass et al., 2017; Brüning & Manzey, 2018; Ellenbogen & Meiran, 2008; Oberauer, 2009; Oberauer, Souza, Druey, & Gade, 2013; Schubert & Strobach, 2018). Crucially, since working memory is limited in capacity (e.g. Baddeley, 2003 and many more), the assumption that the task-order set is processed in working memory characterizes task-order coordination as a resource dependent process. As a result, increasing working memory load should affect the efficiency of the task-order set processing. This assumption was tested in Study 1 of the present dissertation.

A second assumption of the model concerns the exact content of the task-order set. In particular, in addition to order information, it is also necessary to maintain and process specific component task information during dual-task processing, e.g. stimulus information or stimulus-response (S-R) mappings (Strobach & Schubert, 2017; see also Ellenbogen & Meiran, 2008). However, so far it is still an open question of how these two different types of information are mentally organized during dual-task processing. The model assumes that the task-order set only represents information about the sequence of task processing but not information about the specific component tasks. This specific component task information is represented separately from the task-order set by the task sets of the component tasks. This notion of the separate representation of these two types of information is in line with the idea that task information is represented in an agglomerated fashion during multitasking situations with different informational components stored by distinct representations (Hübner et al., 2001; Meiran et al., 2008). As a consequence of this separate representation, it should be possible to change or adjust task-order and specific component task information individually during dual tasking. The assumption of separate representations for task-order and specific component task information was investigated in Study 2 of the present dissertation.

Furthermore, the model's third assumption bears on the question of how task-order coordination is implemented by the human brain. More specifically, the model assumes that the dlPFC - a pivotal brain structure for executing cognitive control processes (e.g. Brass et al., 2005) - is causally involved in task-order coordination processes. So far, preliminary evidence from neuroimaging studies suggests that this brain region may play a crucial role for the implementation of task-order coordination (Schubert & Szameitat, 2003; Stelzel et al., 2008; Szameitat et al., 2006; Szameitat et al., 2002). However due to the nature of the imaging approach applied in these studies, causal conclusions about the role of the dlPFC for task-order coordination are still pending. In Study 3, I employed transcranial magnetic stimulation (TMS) to investigate the causal link between dlPFC activation and task-order coordination.

A fourth assumption included in the model concerns the role of different order criteria for task-order coordination processes in dual-task situations. More specifically, the demand on task-order coordination processes should be dependent on the order criterion determined, for example, by the specific task instruction. Usually, in studies investigating task-order coordination, participants are instructed to match their processing order to a changing external order criterion, i.e. the order of stimulus presentation (Schubert, 2008). This might increase demands on task-order coordination since participants to monitor the order of stimuli, make a decision about the correct task order and adjust the order of task processing accordingly by selecting the appropriate task-order set (Schubert & Szameitat, 2003; Stelzel et al., 2008). However, so far it is still an open question of how other task-order criteria may or may not affect the demands on task-order coordination processes. In fact, less strict order criteria that rely on free order choices and self-organization may change or even reduce the demands on task-order coordination. This assumption was tested in Study 4 of the present dissertation.

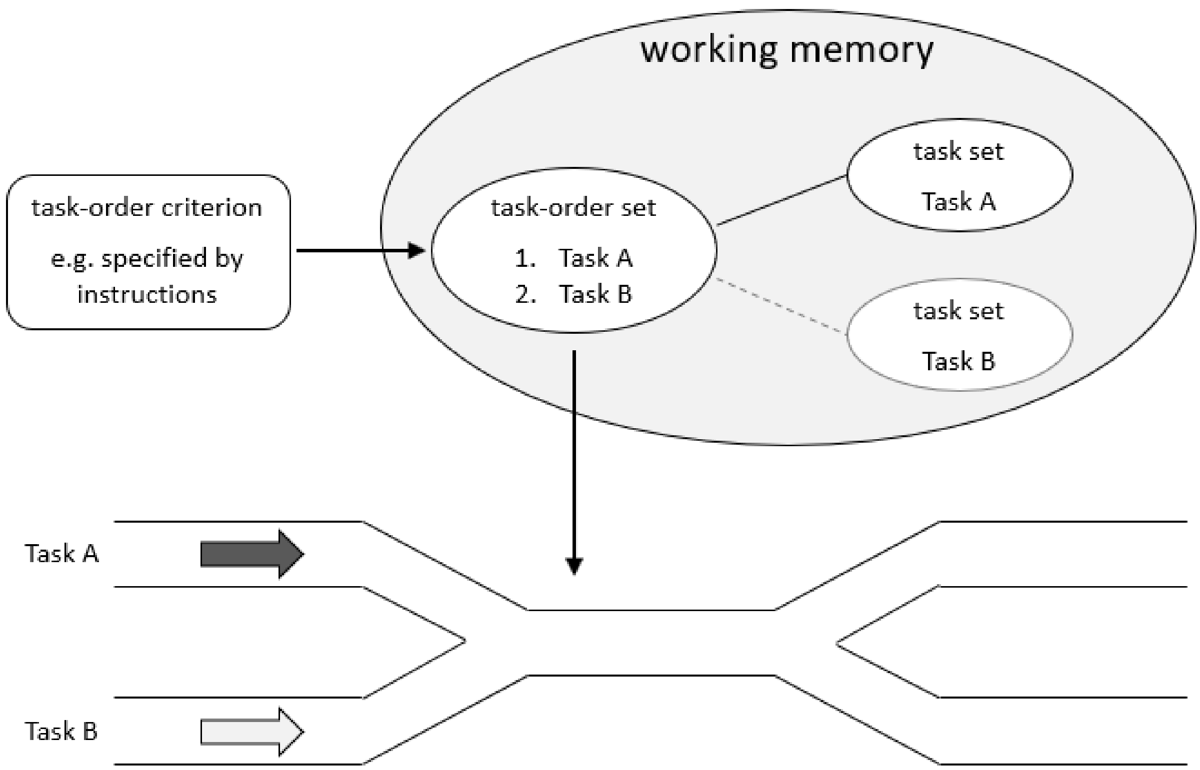

Figure 1: Task-order at the bottleneck stage in dual-task situations is regulated by task-order coordination processes that rely on a task-order set containing information about the sequence of task processing. The task-order set is maintained and processed in working memory separately to specific component task information and guides task order by sequentially activating the task sets of the component tasks. In this example, the task-order set specifies that Task A is processed first, and Task B is processed second. Solid and dashed lines indicate the sequence of task set activation by the task-order set. Additionally, the selection of the task-order set is dependent on an order criterion which is given by, for example, task instructions.

To sum up, the model I tested in my dissertations assumes that task-order coordination processes are necessary in dual-task situations due to a bottleneck that requires the serial processing of tasks. These task order coordination processes rely on a task-order set which specifies the processing sequence during dual-task processing. Furthermore, the model contains the following four assumption: (1) The task-order set is maintained and actively processed in working memory. (2) The task-order set only contains information about task order and does not specify information about the particular component tasks. (3) The implementation of task-order coordination processes is causally related to activity of the dlPFC, a brain region involved in cognitive control processes. (4) The demands on task order coordination depend on the particular task instructions determining a specific order criterion. The aim of the current dissertation was to test these four assumptions. Before I will summarize the empirical studies that are part of this dissertation, in the next chapter I will give an overview over the applied methods as well as the research questions investigated in each of the studies.

5. Method and research questions

5.1 Characteristics of the behavioral paradigm applied in the present dissertation

In order to test the assumptions of the model I introduced in the previous chapter, I employed a PRP-like dual-task paradigm with changing stimulus order (De Jong, 1995; Szameitat et al., 2002). For this purpose, I applied an auditory and a visual task as component tasks. Importantly, these tasks employed target stimuli with clearly defined on- and offsets. As a result, I was able to flexibly manipulate the order of stimulus presentation from trial to trial within random-order dual-task blocks. In contrast to traditional PRP approaches, this allows for the occurrence of same-order and different-order trials and, thus, the investigation of active and order set based task-order coordination processes (Schubert, 2008). In each trial, one auditory (e.g. a sine wave tone) and one visual (e.g. a digit) target stimulus were presented one after the other separated by an SOA of +/-200 ms (with a positive SOA indicating an AUD→VIS trial and a negative SOA indicating a VIS→AUD trial). This specific SOA of +/-200 ms was chosen, as, on the one hand, it results in a high temporal overlap between both tasks and, thus, the requirement for task-order coordination processes, while, one the other hand, still allowing for appropriate judgements of the correct stimulus order (Stelzel et al., 2008; Szameitat et al., 2006). This was important, since participants were instructed to respond as fast and accurately as possible to the target stimuli and according to the order of their presentation (for an alternative instruction condition, see Study 4).

Furthermore, I decided to employ relatively simple choice RT tasks (e.g. a digit or sine-wave tone discrimination task) as component tasks rather than, for example, more complex continuous tasks. This approach allowed for an easy manipulation of working memory load during dual tasking. In particular, during dual-task situations, participants have to maintain component task information, e.g. stimulus or S-R mapping information, active in working memory (Schubert & Strobach, 2018). By varying the number S-R mappings in Study 1 of the current dissertation (see Chapter 6), I manipulated this amount of component task information and, thus, working memory load (Kikumoto & Mayr, 2017; Stelzel et al., 2008). By doing so, I tested, whether the active processing of the task-order set is hampered in dual-task situations with high relative to low working memory load.

An additional advantage of implementing choice RT tasks in the applied dual-task paradigm is that this type of component task can be easily changed within a sequence of trials irrespective of the specific task order. In particular, in Study 2 of the current work (Chapter 7), I employed a newly developed dual-task paradigm in which task order as well as the specific component tasks varied randomly from trial to trial. By applying this approach, I tested whether the task-order set and the task sets of the component tasks can be changed individually. Importantly, if this is the case, this would support the assumption that task-order and specific component task information is represented separately.

In addition, in Study 3 and 4 of the current dissertation (see Chapters 8 and 9), I also implemented fixed-order dual-task blocks. In these fixed-order blocks, the order of stimulus presentation remained constant throughout an entire block and participants were informed about the particular task order before the start of each block. Importantly, in these fixed-order blocks the demands on task-order coordination are reduced compared to random-order blocks. This is so because participants know the order of stimulus presentation in advance and can employ a constant scheduling strategy without the requirement to monitor the stimulus sequence and constantly adjust their processing order. In Study 3, I applied these fixed-order blocks as a control condition and compared the effects of TMS over the dlPFC between fixed-order and random-order blocks. This allowed me to test for the causal role of this brain region for the implementation of task-order coordination processes. Similarly, in Study 4, fixed-order blocks served as baseline dual-task condition with low demands on task-order coordination. Comparing performance between fixed-order blocks and random-order blocks, provides an indicator for the effort associated with the employment of task-order coordination processes (Stelzel et al., 2008; Szameitat et al., 2002). Thus, by measuring the performance difference between fixed-order blocks and random-order blocks with different order criteria, I tested whether and how these different criteria modulate task-order coordination processes.

5.2 Transcranial magnetic stimulation

In addition to the behavioral paradigm described above and in order to test the causal role of the dlPFC for task-order coordination, I used TMS in Study 3 of the present dissertation. TMS is a noninvasive brain stimulation method that interferes with cortical information processing in narrowly circumscribed brain regions with high temporal resolution (Bestmann, 2008; Pascual-Leone, Walsh, & Rothwell, 2000). For this purpose, during stimulation a magnetic coil is held over the stimulation site. By inducing an electrical field in the brain area underneath the coil via electro-magnetic induction, TMS can reversibly disturb neural information processing implemented by the stimulated brain region and modulate participants´ task performance (Miniussi, Harris, & Ruzzoli, 2013; Siebner, Hartwigsen, Kassuba, & Rothwell, 2009). In contrast to imaging methods, applying this approach allows for drawing causal conclusions about the role of a specific brain region for cognitive processing.

I applied TMS to the left *inferior frontal junction* (IFJ) – a subregion of the dlPFC. The IFJ is located at the junction of the inferior frontal sulcus and the precentral sulcus and has been associated with task-order coordination processes (e.g. Stelzel et al., 2008; Szameitat et al., 2002) as well as other cognitive control processes (Brass et al., 2005; Derrfuss, Brass, & von Cramon, 2004). I applied TMS online, i.e. while participants performed a random-order dual-task. For stimulation, I used the eXimia Navigated Brain Stimulation System (Nexstim, Helsinki, Finland) with a focal bipulse figure-eight coil (inner winding diameter: 50 mm; outer winding diameter: 70 mm). During the experiment, I applied TMS in trains of four pulses with a frequency of 10 Hz and an intensity of 110 % of the individual's motor threshold. Furthermore, I validated coil positioning using a neuronavigated stimulation approach. In particular, I controlled the position of the coil over the IFJ by means of neuronavigation software employing a Polaris Spectra® 3D Optical tracking unit (NDI, Waterloo, Canada). This tracking unit enables the recording of the real-time position and orientation of the TMS coil with respect to the participant's head. This procedure is based on a coil specific 3D model, the stimulator parameters, and individual's structural brain images. Individual structural scans were acquired beforehand with a 3.0 T Siemens Magnetom Trio-scanner using a 32-channel radiofrequency head coil. For each participant, I located the IFJ at the intersection point of the inferior part of the precentral sulcus and the posterior part of the inferior frontal sulcus (Derrfuss, Brass, von Cramon, Lohmann, & Amunts, 2009). Using this neuronavigated approach, I guaranteed for accurate stimulation of the IFJ throughout the entire experiment. This was also confirmed by an average distance of 1.81 mm between the individuals' IFJ and the peak electric field induced by TMS on the cortical surface.

To test whether the IFJ is indeed involved in task-order coordination I compared dual-task performance after IFJ TMS with dual-task performance in two control conditions. In one of these control conditions no TMS was applied. In addition to this no-TMS condition, in a second control condition TMS was applied over the vertex. I implemented this second control condition in order to rule out that any observed effects of IFJ stimulation may have been caused by confounding non-neural effects of TMS (e.g. tingling sensations on the scalp, noise produced by stimulation, see Jung, Bungert, Bowtell, & Jackson, 2016). For each participant, I located the vertex at the Pz electrode position according to the international 10-20 system. The specific hypotheses regarding the effect of IFJ TMS on dual-task performance will be presented in the summary of Study 3 (Chapter 8).

5.3 Research questions of Study 1, 2, 3, and 4

The model of task-order coordination I described earlier further specifies the cognitive and neural mechanisms underlying active task-order coordination processes by making several assumptions. The aim of the present dissertation was to test these assumptions in a series of four studies. These studies addressed the following research questions:

1. Is the task-order set maintained and actively processed in working memory? Does increasing working memory load during a dual-task situation hamper the processing of the task-order set compared to a situation with low working memory load?
2. Which exact information is represented by the task-order set? Does the task-order set only contain information about the sequence of task processing or does it also contain information about the specific component tasks?
3. Is the dlPFC causally related to task-order coordination processes? Does TMS of the IFJ interfere with task-order coordination processes and hamper performance in a random-order dual-task situation compared with control TMS conditions?
4. How do different order criteria affect task-order coordination? Do random-order dual-task situations with a mandatory and externally determined order criterion pose higher demands on task-order coordination processes compared to dual-task situations with an internally generated order criteria that is based on free order choices?

In the Chapters 6 – 9, I will give a summary of the Studies 1 – 4 and will present the relevant research questions and corresponding hypotheses in more detail.

6. Study 1: Is the task-order set processed in working memory?

6.1 Research question and methods

Previous studies on task-order coordination have found evidence for the processing of a task-order set by observing performance benefits for same-order compared with different-order trials (De Jong, 1995; Luria & Meiran, 2003; Szameitat et al., 2006). While the existence and role of this order set for task-order coordination is well established (see also Hirsch et al., 2018; Steinhauser & Steinhauser, 2018; Strobach, Antonenko, et al., 2018), the locus of its processing is still a matter of debate. Concerning this issue, a core assumption of the model I tested in this dissertation is that working memory plays a crucial role for maintaining and processing the task-order set. In more detail, during the course of a dual-task trial, participants have to select the appropriate task-order set and activate it in working memory. From there, the task-order set then guides the sequence of component task processing. Importantly, as the task-order set is a representation in working memory, its activation is maintained over time, i.e. between consecutive trials. As a result, the task order set can affect performance in the second of two succeeding trials (for a similar account see e.g. Hirsch et al., 2017; Luria & Meiran, 2006). In same-order trials, participants can simply re-apply the task-order set from the preceding trial since it is still active in working memory after its recent implementation. In different-order trials, in contrast, the order set of the previous trial is not appropriate anymore due to the change in task order from trial *n - 1* to trial *n*. As a result, the activation of the task-order set from the previous trial has to be overcome and a new task-order set has to be implemented in working memory. This is more demanding relative to re-applying the task-order set from the previous trial and, thus, results in performance benefits for same-order trials compared with different-order trials.

The notion of active task-order set processing in working memory is supported by other research in the field of multitasking suggesting a prominent role of working memory for maintaining and coordinating multiple elements of information during task processing (Brüning & Manzey, 2018; Law, Trawley, Brown, Stephens, & Logie, 2013; Redick et al., 2016; Todorov, Kubik, Carelli, Del Missier, & Mäntylä, 2018). Studies on dual-task performance, for example, have already shown that component task information (e.g. stimulus information or S-R mapping information) is implemented in working memory during the processing of a dual-task trial (Ellenbogen & Meiran, 2008; Maquestiaux, Hartley, & Bertsch, 2004; Schubert &

Strobach, 2018; Strobach et al., 2014). These theoretical considerations and empirical observations make it plausible that, in addition to component task information, also the task-order set is actively processed in working memory. Importantly, according to this assumption, task-order coordination processes should rely on available working memory resources

However, an empirical test of this assumption remains yet to be done. This is especially important, because alternatively, rather than active processing in working memory, performance benefits for same-order relative to different-order trials may reflect merely a consequence of automatic priming processes in long-term memory (Logan, 1988, 2002; Schneider & Logan, 2005; see also Hommel & Eglau, 2002; Mayr & Bryck, 2005; Waszak, Hommel, & Allport, 2003). According to Logan’s instance theory (1988, 2002; see also Hommel, 1998; Hommel, Proctor, & Vu, 2004), task features, such as the order of the processed stimuli or of the processed motor response, are automatically encoded and stored as an integrated episodic trace in long-term memory during task processing. Future events that share features with the stored memory trace can result in its automatic retrieval. This retrieval of memory traces from prior task experience can then facilitate current task performance. Thus, repeating the task order of the preceding trial may activate task-order information in long-term memory, which then would result in the performance benefits for same-order relative to different-order trials. As a result, according to this perspective, task-order coordination would not be dependent on active processing in working memory.

A crucial consequence following the assumption of active task-order set processing in working memory is that task-order coordination processes should rely on available working memory resources. Research has shown that working memory can only maintain a limited amount of task information (Baddeley, 2003; Cowan, 2010; Oberauer, 2010). Thus, increasing working memory load and reducing the amount of available working memory resources should hamper the maintenance and processing of the task-order set. Consequently, in dual-task situations with high working memory load, the performance benefit for same-order trials relative to different-order trials should be reduced compared to a dual-task situation with low working memory load. This should be the case because under high working memory load, working memory resources should not suffice to maintain the task-order set efficiently between two succeeding trials. As a result, in same-order trials, participants cannot simply re-apply and capitalize on the task-order set of the preceding trial but instead are again required to upload an order set into working memory (despite a repeated task order). Consequently, the processing difference between same-order and different-order trials should be reduced, resulting in decreased RT differences between both trial types under high compared with low working memory load.

I tested this assumption in a series of two experiments. In both experiments, participants performed a random-order dual task under a low and a high working memory load condition. In Experiment 1, I varied the amount of component task information to be held active in working memory during dual-task processing (Kikumoto & Mayr, 2017; Schubert & Strobach, 2018; Stelzel et al., 2008). To this end, I applied dual-task blocks with two S-R mappings for each task (low load condition) and dual-task blocks with four S-R mappings for each task (high load condition). However, this approach yielded unequal numbers of stimulus and response repetitions between low and high load conditions, which could also explain reduced performance benefits for same-order trials in the latter condition. To exclude that reduced performance benefits for same-order trials can be entirely accounted for by this confound, in Experiment 2, I decided to employ a different working memory manipulation. For this purpose, I introduced an additional working memory updating task into the applied dual-task paradigm. In blocks with high working memory load, I presented arithmetical stimuli (a '+' sign or a '-' sign) as the fixation mark at the beginning of each trial. Based on the presented operator, participants had to constantly perform a continuous mental calculation. As a result, in the high load condition, participants had to permanently maintain and manipulate arithmetical information in working memory in addition to the information relevant for the dual task at hand. In the low load condition, I also presented these operators as a fixation mark but instructed participants to simply monitor the sequence of operators throughout the entire block without performing mental calculations. As result, working memory load should be reduced in low-load blocks compared with high-load blocks

6.2 Results and Discussion

In Experiment 1, I tested the effect of increased working memory load on performance benefits for same-order versus different-order by varying the amount of component task information between low and high load conditions. As a result, I demonstrated that the performance benefit for same-order trials compared with different-order trials varied as a function of working memory load as was predicted by the tested model of task-order coordination (for statistical results please refer to the original research article in Appendix A). For task 1, in the low load condition, I found the typical performance benefit for same-order trials (mean [*m*] = 955 ms) compared with different-order trials (*m* = 1022 ms) indicating the processing and maintenance of the task-order set in working memory (see Figure 2). In the high load condition, this benefit was heavily reduced; RT 1 did not differ significantly between same-order (*m* = 1175 ms) and different-order trials (*m* = 1183 ms). Also, RTs of task 2 were faster in same-order (*m* = 1100 ms) compared with different-order trials (*m* = 1163 ms), when demands on working memory were low. Similar to the results for task 1, after increasing working memory load, this benefit for same-order trials could not be replicated; there was no significant difference to be found between same-order (*m* = 1378 ms) and different-order trials (*m* = 1390 ms). This result is in line with the assumption that the task-order set cannot be maintained efficiently when working memory is pushed to its limits due to high load.

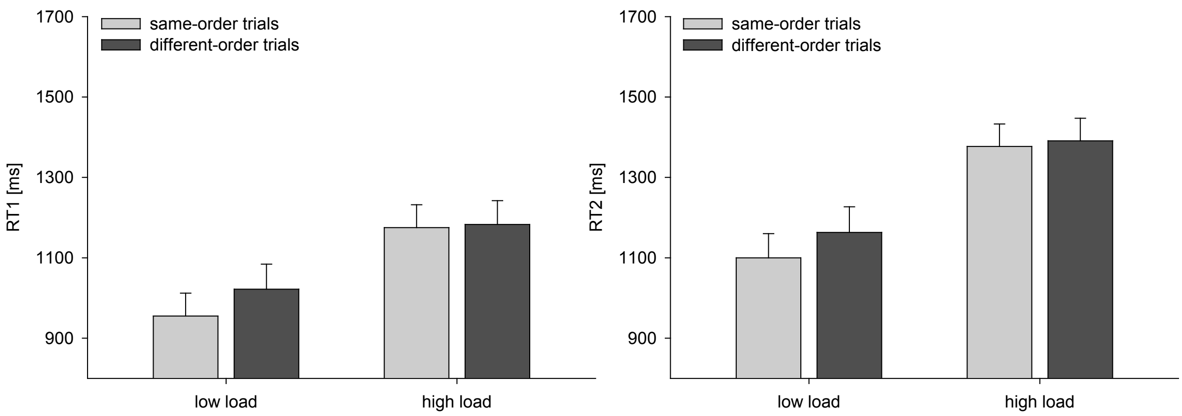

Figure 2: Mean RTs for task 1 and task 2 as a function of trial type and working memory load for Experiment 1 of Study 1. Error bars reflect the standard error of the mean. Left panel: reaction times for task 1 (RT 1), right panel: reaction times for task 2 (RT 2).

In Experiment 2, I tested the reliability of these results by introducing an additional working memory updating task. Importantly, in this second experiment I replicated the results of the first experiment (see Figure 3). In the low load condition, I found faster RT 1 in same-order trials (*m* = 1157 ms) compared with different-order trials (*m* = 1272 ms). Under the high load condition, no such benefit for same-order (*m* = 1302 ms) versus different-order trials (*m* = 1339 ms) could be observed in RT 1. Analogously for task 2, during the low load condition RTs were faster in same-order trials (*m* = 1319 ms) compared with different-order trials (*m* = 1425 ms), whereas in the high load condition I did not find RT differences between same-order (*m* = 1490 ms) and different-order trials (*m* = 1507 ms).

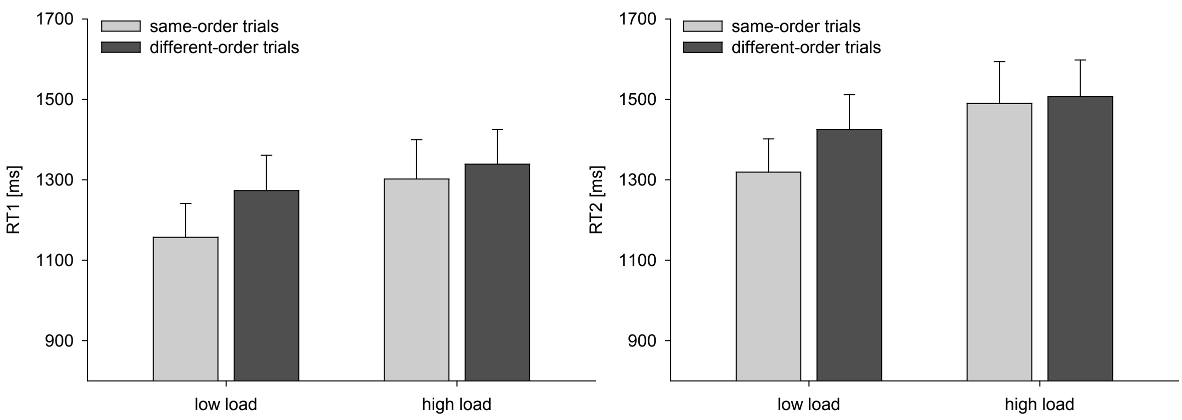

Figure 3: Mean RTs for task 1 and task 2 as a function of trial type and working memory load for Experiment 2 of Study 1. Error bars reflect the standard error of the mean. Left panel: reaction times for task 1 (RT 1), right panel: reaction times for task 2 (RT 2).

To sum up the findings of Study 1: In two experiments, I found a modulation of the performance benefits for same-order trials by varying working memory load during a random-order dual-task situation. While in the low load condition, performance was facilitated in same-order trials compared with different-order, increasing working memory demands in high load conditions resulted in a reduction of these benefits. Importantly, this contradicts the assumption that performance benefits for same-order trials occur due automatic priming in long-term memory (Logan, 2002; see also Hommel, 2004). Instead, this result is in line with the assumption that the task-order set is actively maintained and processed in working memory (see also Hirsch et al., 2018; Luria & Meiran, 2006) and that increasing working memory load results in hampered order set processing.

The findings of Study 1 are in agreement with the general assumption that working memory plays an important role for multitasking by representing relevant task information and making it available for cognitive operations and actions (Law et al., 2013; Redick et al., 2016; Todorov et al., 2018). In line with this assumption and based on the findings of Study 1, I can conclude that also task-order information is stored and manipulated in working memory during dual-task processing. This perspective on the role of working memory for dual-task performance is also in agreement with broader models that highlight the general involvement of working memory in goal directed behavior (Brass et al., 2017; Oberauer, 2009, 2010). I will give an outlook on how my model of task-order coordination may relate to these models in the General Discussion of this dissertation (Chapter 10).

7. Study 2: Which exact information is stored in the task-order set?

7.1 Research question and methods

In random-order dual tasks, performance benefits occur for same-order trials compared with different-order trials (De Jong, 1995; Steinhauser & Steinhauser, 2018; Szameitat et al., 2006). Although this reliable phenomenon has been commonly accepted as evidence for the processing of a task-order set, fundamental characteristics of this order set are still unknown. For example, so far, it is still an open question which exact information is represented by the task-order set and how this information relates to other types of information that have to be maintained concurrently during dual-task processing. In particular, in addition to the specific order information, participants also have to maintain and implement the task sets of the component tasks constituting the dual-task at hand (Ellenbogen & Meiran, 2008; Strobach & Schubert, 2017). In contrast to the task-order set, these task sets do not represent order information but instead they contain specific component task information, such as stimulus and response information and S-R mappings. In Study 2 of the current dissertation, my aim was to investigate how these different components of information, i.e. task-order information and specific component task information, are mentally organized during dual-task processing.

The model of task-order coordination I tested in this dissertation assumes that the task-order set only contains information about the sequence of task processing but not information about the specific component tasks. In other words, task-order information, on the one hand, is represented by the task-order set, while specific component task information, on the other hand, is represented separately by the task sets of the component tasks. For illustrative purposes, one might imagine a random-order dual task consisting of a tone and a digit discrimination task. In the auditory component task participants are instructed to respond to tones with different pitches by pressing response buttons with the fingers of their left hand, while in the visual component task they are asked to respond to different digits by pressing response buttons with the fingers of their right hand. The proposed model of task-order coordination assumes that, in this case, the order set only specifies the order in which to perform both component tasks (e.g. perform the auditory component task first and the visual component task second). However, it would not specify how to act upon the presented target stimuli (e.g. respond to the low pitch tone by pressing the left response button with your left middle finger and respond to the letter ‘K’ by pressing the right response button with your right index finger). This specific component task information is instead specified by the task sets of component tasks containing stimulus, response and S-R mapping information. To sum up, the model I tested assumes that task-order and specific task information is stored separately by distinct representations, i.e. the task-order set and the task sets of the component tasks, respectively.

Indirect evidence for the assumption of separate representations for task-order and component task information stems, for example, from a neuroimaging study of Stelzel et al. (2008). In this study, the authors demonstrated that task-order and component task information is stored in and implemented by distinct brain regions. Also, earlier behavioral studies on task-order coordination have already theorized that the task-order set stores order information separately from specific component task information (Luria & Meiran, 2003, 2006). Importantly, the assumption of such a separate representation of different types of information in dual tasks is also in line with a perspective according to which multitasking situations are represented in an agglomerated fashion, i.e. with different informational components represented by distinct representations (Meiran et al., 2008; Rangelov, Töllner, Mueller, & Zehetleitner, 2013).

However, so far, a direct test of this assumption has not been conducted yet. This is especially important because, as hypothesized by other groups (e.g. Hirsch et al., 2017; Hirsch et al., 2018), it is also plausible that the task-order set not only contains order information but also integrates specific component task information. This would mean that the order set not only represents the sequence of task processing but also the particular stimuli, responses and S-R mappings for both component tasks. For the example of the dual task given in the previous paragraph, the task-order set would, on the one hand, specify which task to perform first and which task to perform second *and*, on the other hand, which buttons to press as a response to the presented target stimuli (e.g. respond first to the low pitch tone by pressing the left response button with your left middle finger and second to the letter ‘K’ by pressing the right response button with your right index finger). In other words, according to this perspective, the task-order set fuses together task-order information and specific component task information into one representation containing different types of information concurrently. Such a view would be in line with accounts that suggest that that multitasking situations are represented in an integrated fashion, i.e. by a representation that jointly specifies all relevant informational components necessary for the task at hand (e.g. Philipp & Koch, 2010; Vandierendonck, Christiaens, & Liefooghe, 2008)

To put both assumption to the test, I developed a new dual-task paradigm. In this paradigm, I implemented a variable task order as well as randomly changing component tasks (for dual-task situations with changing component tasks but constant task order, see Hirsch et al., 2017; Hirsch et al., 2018). I compared the performance benefit for same-order trials relative to different-order trials under the condition of repeated and changed component tasks. Importantly, if the task-order set only contains information about task order without further specifying the component tasks, I should observe a performance benefit for same-order relative to different-order trials irrespective of whether the specific component tasks have repeated or changed relative to the preceding trial. This is so, because the order set would only specify the sequence of processing for both tasks. As a result, changing the particular component tasks (while repeating task order) does not require the activation of a new task-order set. Consequently, in same-order trials participants can re-apply the task-order set of the preceding trial under the condition of repeated component tasks as well as under the condition of changed component tasks. Only in different-order trials, when task order is reversed relative to the preceding trial, participants have to activate a new task-order set, resulting in increased RTs compared to same-order trials. In sum, if task-order and component task information is represented separately, I should find performance benefits for same-order versus different-order trials irrespective of task repetitions or changes.

If, alternatively, the task-order set contains task-order information and specific component task information in an integrated fashion, I should observe a performance benefit for same-order compared with different-order trials only when the component tasks repeat relative to the preceding trial, but not when the component tasks change. This is so because the task-order set would not only specify the processing order but also the component tasks of the current trial. When the task-order and the component tasks are repeated relative to the preceding trial, participants can re-apply the task-order set of the preceding trial. When, however, the component tasks change, the task-order set of the previous trial does not specify the correct component tasks anymore. Consequently, a new task-order set has to be activated – even for the case of a repeated task order relative to the preceding trial. Thus, when the component tasks have changed relative to the preceding trial in both same-order and different-order trials a new task-order set has to be activated. Consequently, in the case of changed component tasks, there should be no RT difference between the two trial types.

I tested these two predictions against each other in a series of three experiments. In Experiment 1, I applied a random-order dual task consisting of one auditory and two visual component tasks. In the auditory component task, participants had to discriminate between two tones with different pitches. In the visual component task participants had to discriminate either between two digit or two letter stimuli. In each trial, I presented one auditory and one visual target stimulus. The order of stimuli as well as the specific visual component task varied randomly from trial to trial, yielding same-order trials and different-order trials with either a repeated or changed visual component task relative to the preceding trial. In Experiment 2, I tested whether the results of Experiment 1 can be generalized to a dual-task situation with two changing auditory rather than two visual component tasks. To this end, I applied one visual component task (the digit task from the first experiment) and two auditory component tasks. These two auditory component tasks were either a pitch discrimination task or timbre discrimination tasks. In Experiment 3, I tested whether evidence for a separate representation of task-order and component task information can also be found when demands on maintaining component task information in working memory are increased. This test was necessary, as in the first two experiments participants had only to maintain three component tasks active resulting in relatively low demands on working memory. These low demands may have allowed participants to represent order and component task information separately. However, increasing these demands may force participants to use a more parsimonious form of representation and integrate task-order and component task information. To test whether order and component task information is also represented separately when demands to maintain component task information active in working memory are increased, I applied a random-order dual task with two visual (a digit and a letter task) and two auditory (a pitch and timbre task) component tasks in Experiment 3. This approach yielded same-order and different-order trials with repeated component tasks, with one changed component task (either the visual or the auditory component task), and with two changed component tasks (both the visual and the auditory component task). Based on the assumption that the task-order set only contains order information but not specific component task information, I expected performance benefits for same-order compared with different-order trials in the condition of repeated as well as in the condition of changed component tasks in all three Experiments.

7.2 Results and Discussion

In Experiment 1, I applied a random-order dual task with a changing visual component task. Importantly, as can be seen in Figure 4, I found the typical performance benefits for same-order compared with different-order trials irrespective of a repeated or a changed visual component task: In the condition of a repeated visual component task, RTs for task 1 were faster in same-order trials (*m* = 1006 ms) than in different-order trials (*m* = 1069 ms). The same holds true for trials with a changed visual component task with shorter RT 1 for same-order trials (*m* = 1046 ms) compared with different-order trials (*m* = 1095 ms). I observed an identical pattern for task 2. The performance benefit for same-order trials (*m* = 1084 ms) relative to different-order trials (*m* = 1151 ms) in the condition of a repeated visual component task was replicated in the condition changed visual component task as was indicated by faster RT 2 for same-order trials (*m* = 1131 ms) in comparison with different-order trials (*m* = 1183 ms; for statistical results please refer to the original research article in Appendix B).

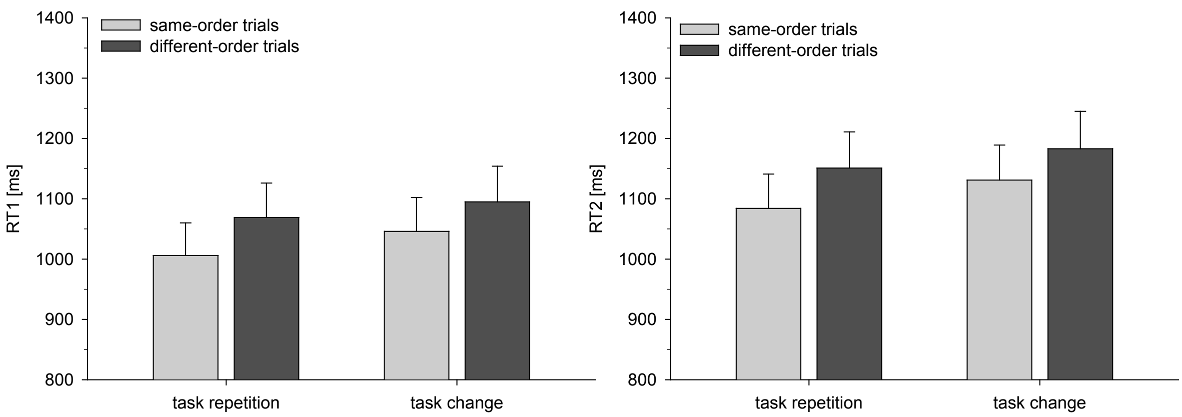

Figure 4: Mean RTs for task 1 and task 2 as a function of trial type and repetition versus change of the visual component task in Experiment 1 of Study 2. Error bars reflect the standard error of the mean. Left panel: RTs for task 1, right panel: RTs for task 2.

In Experiment 2, I replicated these results in a dual-task situation with a changing auditory but constant visual component task (see Figure 5). More specifically, I observed faster RTs for task 1 in same order trials (*m* = 1022 ms) relative to different-order trials (*m* = 1092 ms) in the condition of a repeated auditory component task. Most importantly for the current research question, I also found these performance benefits in task 1 when the auditory task had changed relative to the preceding trial indicated by shorter RT 1 in same-order trials (*m* = 1091 ms) compared with different-order trials (*m* = 1143 ms). The findings in task 2 showed a similar pattern: RT 2 was shorter in same-order trials (*m* = 1128 ms) compared with different-order trials (*m* = 1198 ms) when the auditory task had been repeated. Importantly, also under the condition of a changed auditory task, I found faster RT 2 for same-order trials (*m* = 1208 ms) compared with different-order trials (*m* = 1257 ms).

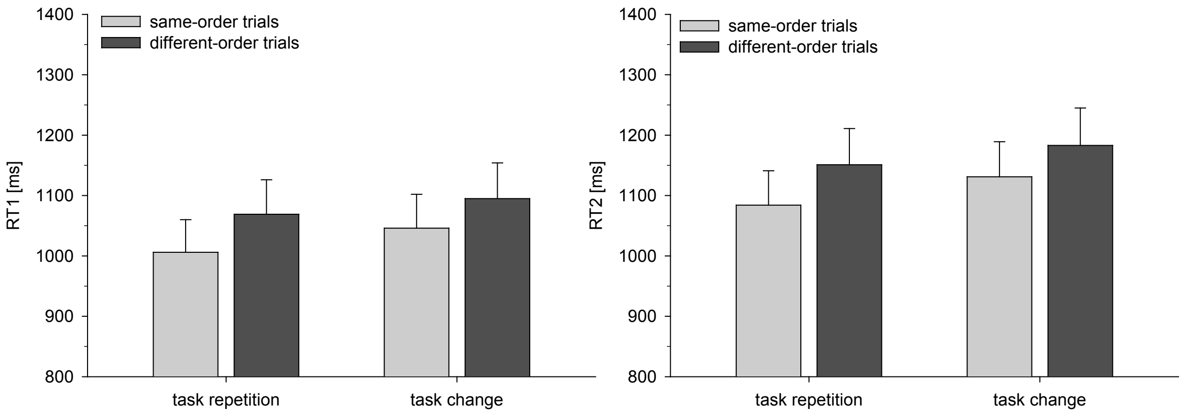

Figure 5: Mean RTs for task 1 and task 2 as a function of trial type and repetition versus change of the auditory component task in Experiment 2 of Study 2. Error bars reflect the standard error of the mean. Left panel: RTs for task 1, right panel: RTs for task 2.

In Experiment 3 (see Figure 6), I also found evidence for the separate representation of task-order and specific component task information – even under the condition of increased demands to maintain four (rather than three) task sets active in working memory. For task 1, I found faster RTs for same-order trials compared with different-order trials in the condition of repeated component tasks ($m_{same\text{-}order}$ = 938 ms, $m_{different\text{-}order}$ = 1041), in the condition of one changed component task ($m_{same\text{-}order}$ = 984 ms, $m_{different\text{-}order}$ = 1087), as well as under the condition of two changed component tasks ($m_{same\text{-}order}$ = 1055 ms, $m_{different\text{-}order}$ = 1099). Also, for task 2, I observed performance benefits for same-order trials indicated by faster RT 2 relative to different-order trial when component tasks were repeated ($m_{same\text{-}order}$ = 943 ms, $m_{different\text{-}order}$ = 1058), when one component task changed ($m_{same\text{-}order}$ = 1003 ms, $m_{different\text{-}order}$ = 1107) and when two component tasks changed ($m_{same\text{-}order}$ = 1084 ms, $m_{different\text{-}order}$ = 1125).

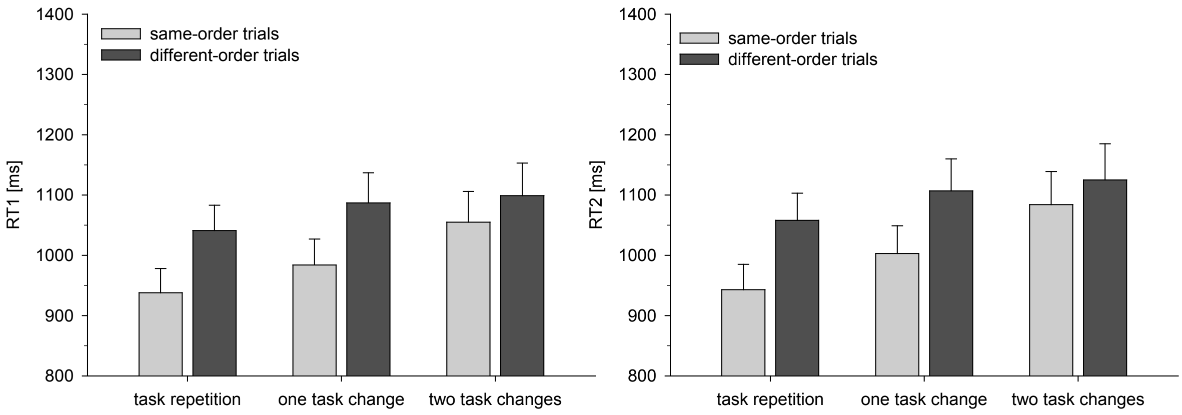

Figure 6: Mean RTs for task 1 and task 2 as a function of trial-type and repetition versus change of one or two component tasks in Experiment 3 of Study 2. Error bars reflect the standard error of the mean. Left panel: RTs for task 1, right panel: RTs for task 2.

To conclude, the results of the three experiments in Study 2 are in line with the assumption of the tested model that the task-order set only contains task-order information but not specific component task information. This was indicated by the observation of performance benefits for same-order trials compared with different-order trials irrespective of repeated or changed component tasks. This pattern of results suggests that - even when the component tasks change – participant can re-apply the task-order set of the previous trial indicating that it only specifies the processing order of tasks but not the component tasks. In contrast, if the task-order set would also contain specific component task information, I should have observed no performance benefit for same-order trials compared with different-order trials after a changed component task. However, as this is not the case, I can reject the assumption that the task-order set integrates task-order and component task information. Instead, my results are in line with the assumption of a separate representation of task-order and component task information.

Importantly, I found evidence for the separate representation of both types of information irrespective of whether the visual (Experiment 1) or the auditory (Experiment 2) component task changed, which suggests that the particular task composition does not affect the organization of task-order and task specific information. Furthermore, the separate representation of task-order and component task information also persists when demands to maintain task information in working memory are increased. This as was indicated by the results of Experiment 3, in which participants had to maintain information of four component tasks active in working memory.

The notion of separate representations for task order on the one and component tasks information on the other hand has already been suggested in previous studies on task-order coordination (Luria & Meiran, 2003, 2006; Sigman & Dehaene, 2006). For example, Luria and Meiran (2006) assumed that, while performing a dual task, participants first activate the task-order set, which then guides the sequence of task processing by sequentially activating the task sets of the component tasks. Similarly, an imaging study of Stelzel et al. (2008) demonstrated that information about task order and information about the component task characteristics, such as stimuli and motor responses, is represented by different brain regions. In this study, the authors independently varied demands on task-order coordination as well as component task difficulty. They showed that increasing component task difficulty resulted in increased brain activation in posterior parts of the inferior frontal sulcus and the anterior insula, whereas increasing the demands on task-order coordination, on the other hand, resulted in increased neural activation in more anterior parts of the inferior frontal sulcus and the middle frontal gyrus. According to the authors, the notion of distinct brain regions that represent different types of information, i.e. task-order and component task information, implies their separate representation. So far, however, no study has provided direct evidence for the notion that the task-order set only contains order information, whereas specific component task information is stored separately. By introducing changing component tasks in addition to a random task order, I tested and, importantly, confirmed this assumption.

To sum up, in Study 2 of my dissertation I demonstrated that the task-order set only contains information about task order but not about the specific component tasks. In fact, component task information seems to be stored separately from task-order information. This contradicts the assumption that the task-order set integrates task-order and specific component task information (e.g. Hirsch et al., 2017; Hirsch et al., 2018). Overall, the observation that task-order and specific task information is stored separately by different representations also contributes to the questions of how to define a task constituted by multiple task components in multitasking situation (Koch et al., 2018). I will get back to this issue in the General Discussion (Chapter 10) of the current dissertation.

8. Study 3: Is the dlPFC causally involved in task-order coordination?

8.1 Research question and methods

Preliminary evidence for the notion that the dlPFC may play an important role for neuronally implementing task-order coordination processes stems from fMRI studies applying dual-task situations with random task order (Schubert & Szameitat, 2003; Stelzel et al., 2008; Szameitat et al., 2002). In the study of Szameitat et al. (2002), for example, the authors applied a dual task in fixed-order and random-order blocks and contrasted performance as well as neural activation between both blocks. They found decreased performance in random-order compared with fixed-order blocks. Importantly, this decrease in performance during random-order blocks was accompanied by increased brain activation in a frontoparietal network. Focal peaks of activation were found close to the IFJ at the intersection of the lower part of the precentral sulcus and the inferior frontal sulcus. This brain region has been theorized to play a pivotal role in tasks that require the execution of cognitive control processes, such as the Stroop or the task switching paradigm (Brass et al., 2005; Brass et al., 2017; Derrfuss et al., 2005; Dove, Pollmann, Schubert, Wiggins, & Von Cramon, 2000).

According to Szameitat et al. (2002), the pattern of increased brain activation in random-order relative to fixed-order blocks is in line with the assumption that the dlPFC is relevant for implementing task-order coordination. As in fixed-order blocks the order of stimuli remains constant, participants can prepare the task order in advance and employ a constant scheduling strategy. In random-order blocks, in contrast, demands on task-order coordination are increased as the order of stimuli varies randomly and, due to instruction, participants have to constantly adjust their processing order. Thus, increased neural activation in this block type suggests the recruitment of the IFJ for implementing task-order coordination. Despite this evidence, causal conclusions about the IFJ's role for task-order coordination cannot be drawn based on these imaging studies due to the correlational nature of the applied fMRI method (Logothetis, 2008). To tackle this issue, I employed TMS in Study 3 of the current work. Since TMS can interfere with neural information processing, applying this method allows for causal conclusions about the relation between a cognitive process and a circumscribed brain region (Pascual-Leone et al., 2000; Siebner et al., 2009).

Participants performed a dual task in fixed-order and random-order blocks. Trials from random-order blocks were further subdivided into same-order and different-order trials. This was done, in order to test whether TMS of the IFJ might distinctively affect performance in both trial types (Szameitat et al., 2006; see General Discussion). I applied TMS online, i.e. while participants performed the dual task. To guarantee that TMS only affects task-order coordination processes, I implemented an order cue with a cue-target interval of 600 ms before the onset of the target stimuli (De Jong, 1995). This order cue indicated the order of stimuli for the current trial. The aim of presenting this order cue was to temporally isolate task-order coordination processes from other processes that are specific for component task processing, such as perceptual processing or response selection. These processes should only occur after the presentation of the target stimuli. TMS was administered after the cue onset and before the presentation of the target stimuli. Since stimulation finished 200 ms before the first stimulus was presented and perturbing effects of individual TMS pulses typically last for between 80 and 120 ms (Miniussi et al., 2013), this approach made any effects of TMS on processes other than task-order coordination rather unlikely.

In Experiment 1, I assessed dual-task performance in three TMS conditions: IFJ TMS, when TMS was administered to the IFJ, vertex TMS, when TMS was administered to the vertex, and no TMS, when no TMS was applied. Importantly, if the IFJ is indeed causally related to task-order coordination, TMS of this brain region should disturb dual-task performance compared to control conditions in trials from random-order blocks, i.e. same-order trials and different-order trials. In trials from fixed-order blocks, on the other hand, IFJ TMS should have no effect on dual-task performance, since demands on task-order coordination are rather low.

In addition, in Experiment 2, I tested whether the causal function for task-order coordination is specific for the IFJ or whether other prefrontal brain regions might also be causally related to task-order coordination. For this purpose, I applied TMS to the pre-supplementary motor area (preSMA) during dual-task processing. I chose this specific brain region for three reasons: First, the preSMA also shows increased brain activation during dual-task compared to single-task situations (Szameitat et al., 2002; see also Schubert & Szameitat, 2003). Second, in studies using TMS, the preSMA has been causally associated with bottleneck processing (Soutschek, Taylor, & Schubert, 2016). And third, studies on the primate brain (Shima & Tanji, 1998; Tanji & Shima, 1994) indicate that the preSMA may be involved in the planning and temporal organization of multiple movements. Importantly, based on the assumption that the causal function for task-order coordination is specific for the IFJ, I did not expect any effects of preSMA TMS on dual-task performance.

8.2 Results and Discussion

In Experiment 1, I compared dual-task performance in trials from fixed-order blocks and trials from random-order blocks, i.e. same-order and different-order trials (detailed statistical results can be found in the original research article in Appendix C). As can be seen in Figure 7, in fixed-order blocks, when demands on task-order coordination are reduced, there was no effect of IFJ TMS on dual-task performance, neither for task 1 nor for task 2. In random-order blocks, in contrast, when task order varied, IFJ TMS resulted in decreased dual-task performance compared to control conditions. For task 1, in same-order trials, RTs were increased after IFJ TMS (*m* = 890 ms) relative to no TMS (*m* = 814 ms) and vertex TMS (*m* = 809 ms). I observed a similar pattern for RT 1 in different-order trials with slower responses in the IFJ TMS condition (*m* = 932 ms) compared with the no TMS (*m* = 893 ms) and vertex TMS (*m* = 843 ms) condition. Furthermore, I found analogous results for RTs in task 2: In same-order trials, RT 2 was slower after IFJ TMS (*m* = 1067 ms) in comparison to RT 2 after no TMS (*m* = 982 ms) and vertex TMS (*m* = 993 ms). Similarly, in different-order trials, RT 2 was prolonged when TMS was applied to the IFJ (*m* = 1151 ms) relative to the no TMS condition (*m* = 1059 ms) and the vertex TMS condition (*m* = 1036 ms). Thus, overall, TMS of the IFJ resulted in slower RTs compared to control conditions in trials from random-order blocks, when demands on task-order coordination are increased, but not in trials from fixed-order blocks, when demands on task-order coordination are reduced.

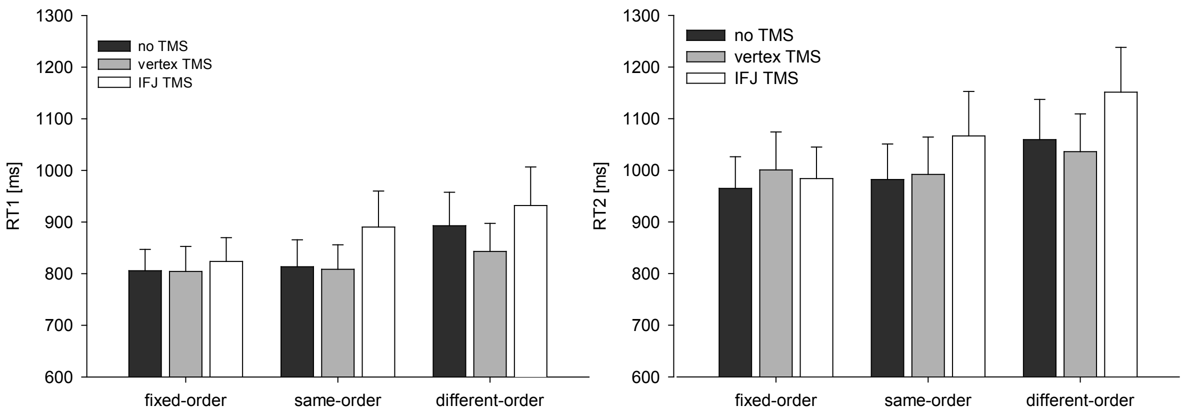

Figure 7: Mean RTs for task 1 and task 2 as a function of trial type and TMS conditions for Experiment 1 of Study 3. Error bars reflect the standard error of the mean. Asterisks indicate significant differences between TMS conditions. Left panel: RTs for task 1, right panel: RTs for task 2.

However, as I applied TMS after the onset of the order cue and before the presentation of the target stimuli, it might be that - at least theoretically – the effects of IFJ TMS in random-order blocks can be attributed to disturbed cue processing rather than impaired task-order coordination processes. In order to eliminate this potential alternative explanation, I applied a control task subsequently to the dual-task situation. For this purpose, I presented additional random-order blocks and asked participants to respond to the order of tasks as it was signaled by the instructional order cue. Hence, in contrast to performing a dual task in the correct order, in the control task participants were only required to process and respond to the order cue at the beginning of each trial. By doing so, I was able to test whether IFJ TMS interferes with cue processing. If this should indeed be the case, I should find decreased performance, i.e. prolonged RTs and increased error rates, in the control task after IFJ TMS compared to control conditions. If, in contrast, IFJ TMS does not affect performance in the control task, I can conclude that TMS of this brain region does not interfere with cue processing. Importantly, I showed that TMS of the IFJ did not result in neither prolonged RTs nor increased error rates in the control task. As a result, data of the control task did not provide any evidence for potential interference with the processing of the order cue due to IFJ TMS. Thus, I can conclude that the effects of IFJ TMS in random-order dual-task blocks can be most probably attributed to disturbed task-order coordination rather than impaired cue processing.

As a next step, I tested whether the causal role for task-order coordination is specific for the IFJ or whether other brain regions might also be recruited for implementing task-order coordination. To this end, in Experiment 2, I applied the same experimental approach as in Experiment 1, with the difference that TMS was applied over the preSMA. Based on previous research, this brain region constitutes a promising candidate structure that might also be causally related to task-order coordination processes (Shima & Tanji, 1998; Soutschek et al., 2016). As in Experiment 1, I used a neuronavigated strategy based on individuals' structural brain scans for locating and validating coil positioning over the preSMA. For each participant, the preSMA was located on the longitudinal fissure, 1 cm anterior to where the verticofrontal line intersects with the outer cortex surface (for a similar location strategy of the preSMA, see e.g. Muessgens, Thirugnanasambandam, Shitara, Popa, & Hallett, 2016). Importantly, in Experiment 2, TMS of the preSMA did not affect dual-task performance. Neither in fixed-order nor in random-order blocks, I found evidence that stimulation of this brain region modulates RTs or any other performance measure compared to both control conditions. Thus, based on this result, I can conclude that the preSMA is not causally involved in implementing task-order coordination processes. Together with Experiment 1, this suggests a specific function of the IFJ for implementing task-order coordination and emphasizes its prominent role for scheduling the sequence of task processing in dual-task situations.

Concerning the discussion, in Study 3, I investigated the causal involvement of the IFJ in task-order coordination during dual-task situations. By applying TMS, I demonstrated that this brain region is indeed recruited for implementing task-order coordination processes. This was indicated by increased RTs for task 1 and task 2 after IFJ TMS compared with control conditions in random-order blocks, when demands on task-order coordination are increased. In fixed-order blocks, when demands on task-order coordination are reduced, TMS of the IFJ did not affect dual-task performance. The observed results confirm the assumption of previous imaging studies that have already suggested a significant role of the dlPFC for task-order coordination (Schubert & Szameitat, 2003; Stelzel et al., 2008; Szameitat et al., 2006). However, based on these studies and due to the correlational nature of the fMRI method, conclusions about the causal role of the dlPFC for task-order coordination were difficult to draw. By means of employing TMS, I addressed this issue and could confirm the assumption that the dlPFC is causally involved in task-order coordination. In addition to confirming the causal role of the dlPFC, I can further specify the functional contribution of this brain region to task-order coordination. In particular, I found effects of IFJ TMS in same-order *and* different-order trials, i.e. both trial types from random-order blocks. Thus, the IFJ seems to implement the cognitive processes that are generally required in random-order blocks, e.g. the matching of one’s processing order to a changing stimulus order. I will get back to this issue in the General Discussion (Chapter 10).

To discuss the results of Experiment 2, TMS of the preSMA did not affect dual-task performance in random-order blocks. This indicates that this brain region may not be causally involved in implementing task-order coordination and, thus, further highlights the prominent and specific role of the IFJ for these processes. Nevertheless, previous studies suggested that the preSMA is also involved in dual-task processing as indicated, for example, by its increased activation during dual-task compared to single-task situations (e.g. Schubert & Szameitat, 2003; Szameitat et al., 2002). But what, if not implementing task-order coordination, is the contribution of the preSMA to dual-task processing? Results of a recent TMS study (Soutschek et al., 2016) suggest that, rather than implementing task-order coordination, the preSMA may resolve conflict between the two component tasks by inhibiting task 2 activation during response selection of task 1. Importantly, the results of Experiment 2 do not contradict this assumption. Instead, I argue that dual-task situations require the interplay of different brain regions in order to guarantee appropriate performance. While the IFJ is involved in implementing task-order coordination processes, the preSMA may play an important role for implementing further processes beyond task-order coordination, such as inhibiting task 2 during bottleneck processing of task 1.

9. Study 4: Do different order criteria affect task-order coordination?

9.1 Research question and methods

Studies employing random-order dual tasks demonstrate the occurrence of active task-order coordination processes (Stelzel et al., 2008; e.g. Strobach, Antonenko, et al., 2018; Strobach, Hendrich, et al., 2018; Szameitat et al., 2002). Importantly, in most of these studies, participants had to adjust their order of task processing to a mandatory and external order criterion (Schubert, 2008). In particular, in these studies, participants received a forced-order instruction, i.e. they were asked to respond to both component tasks according to the changing order of stimulus presentation. However, an important question that arises is whether and how demands on task-order coordination change when participants can employ an internal and more relaxed order criterion that is based on a free order choice.

Based on previous work (De Jong, 1995; Strobach, Hendrich, et al., 2018) it has been hypothesized that adhering to an external and mandatory order criterion in random-order dual-tasks with a forced-order instruction requires additional attentional resources. This is so, because, due to instruction, participants have to judge the temporal sequence of stimuli and match their processing order accordingly. Adhering to an internal order criterion that allows for self-organized task scheduling and free order choices, in contrast, might require less attentional resources since task order can be based upon one's own decision rather than the to be observed stimulus order. According to this view and due to the amount of required attentional resources, demands on task-order coordination should be higher when participants are instructed to schedule their task processing according to a mandatory order criterion, i.e. the stimulus sequence, compared to when they base their processing order on a more relaxed internal order criterion, i.e. their free order choice. This assumption is also in line with studies employing other multitasking paradigms. In particular, studies on task switching have shown that, when participants can freely decide about when to switch and when to repeat a task in a sequence of trials, performance is usually improved compared to when participants have to adhere to an externally determined task sequence (e.g. Arrington & Logan, 2005; Gollan et al., 2014). In analogy, this finding further supports the assumption that demands task-order coordination should be reduced in random-order dual tasks that allow for free order choices compared to situations with externally determined order criteria, such as the given stimulus order.

On the other hand, it has been hypothesized that self-organized task scheduling is an effortful process that relies heavily on executive control functions (Hampshire, Gruszka, Fallon, & Owen, 2008; Kang, DiRaddo, Logan, & Woodman, 2014; Kiesel & Dignath, 2017). Transferring this perspective to dual-task situations, it is highly questionable whether giving participants the opportunity to freely decide about task order indeed reduces demands on task-order coordination. Instead, given the assumption that self-organized task scheduling requires cognitive resources, it is even possible that demands on task-order coordination are increased when participants base their task order on an internally generated order criterion.

To test the effects of different order criteria on task-order coordination, I applied random-order dual-task blocks under two instruction conditions (for a similar approach, see De Jong, 1995). I instructed participants either to respond to both tasks according to the order of stimulus presentation (forced-order instruction) or to decide freely in which order to perform both tasks (free-order instruction). In addition to random-order blocks, I also applied fixed-order blocks, in which the order of stimuli did not vary and in which participants were instructed to use a constant scheduling strategy. I used the performance difference between fixed-order and random-order blocks as an indicator for the degree to which task-order coordination processes are employed (Stelzel et al., 2008; Szameitat et al., 2002). Importantly, if an externally determined and mandatory order criterion poses higher demands on task-order coordination processes relative to an internally generated criterion based on free choice, the performance difference between fixed-order and random-order blocks under the forced-order instruction should be increased compared with the performance difference between fixed-order blocks and random-order blocks under the free-order instruction.

In Experiment 1, I varied the instruction in a between-subjects design, i.e. during random-order blocks one half of participants received the free-order instruction (free-order group), the other half the forced-order instruction (forced-order group). In Experiment 2, I used a yoked within-subject design. For this purpose, in a first session, participants performed random-order blocks under the free-order instruction (free-order session). In addition to performance measures, in this first session I also recorded participants' produced task order on each trial throughout the entire experiment. In a second session, participants performed random-order blocks under the forced-order instruction (forced-order session). Importantly, in this second session, I used the sequence of response orders participants had produced throughout the initial first session as the order of stimulus presentation in the second session.

9.2 Results and discussion

In Experiment 1, I applied different instructions in a free-order group and a forced-order group. First, I compared task-order reversal rates in random-order blocks between both groups in order to verify whether participants adhered to the given instruction. Task-order reversals indicate trials in which participants respond to both tasks in an order that is reversed compared to the order of stimuli. The free-order group produced larger task-order reversal rates (*m* = 35 %) compared with the forced-order group (*m* = 10 %). Thus, in the forced-order group participants relied more on the sequence of stimuli compared with participants in the free-order group indicating that participants complied with their given order instruction. In the next step, I analyzed RTs in fixed-order and random-order blocks. For both groups, I found faster RTs in fixed-order compared with random-order blocks. RT 1 increased from fixed-order blocks (*m* = 909 ms) to random-order blocks (*m* = 1056 ms) in the free-order group, as well as from fixed-order blocks (*m* = 869 ms) to random-order blocks (*m* = 1143 ms) in the forced-order group. Importantly, as can be seen in Figure 8, this increase in RT 1 was larger for the forced-order group (*m* = 274 ms) compared with the free-order group (*m* = 147 ms). I observed similar results for task 2. In the free-order group RT 2 was faster in fixed-order blocks (*m* = 988 ms) relative to random-order blocks (*m* = 1153 ms). Also, in the forced-order group, fixed-order blocks (*m* = 938 ms) yielded faster response times for task 2 compared with random-order blocks (*m* = 1250 ms). However, RT 2 differences between random-order and fixed-order blocks were increased in the forced-order group (*m* = 312 ms) relative to the free-order group (*m* = 165 ms). These results are in line with the assumption that demands on task-order coordination are increased under the forced-order relative to the free-order instruction.

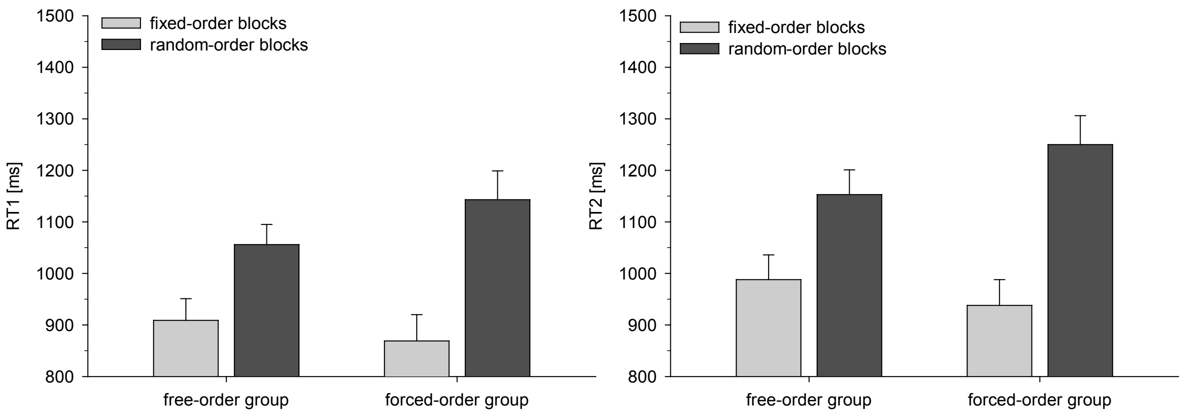

Figure 8: Mean RTs for task 1 and task 2 as a function of block type and instruction group in Experiment 1 of Study 4. Error bars reflect the standard error of the mean. Left panel: RTs for task 1, right panel: RTs for task 2.

Closer inspection of the data, however, revealed that in Experiment 1 participants in the forced-order group switched task order more frequently between consecutive trials than participants from the free-order group. Importantly, this larger order switch frequency might have increased general task difficulty in random-order blocks for the forced-order group and, thus, could also explain the results found in Experiment 1. To exclude this potential confound, in Experiment 2, I applied two dual-task sessions in a yoked design which guaranteed similar order switch rates in random-order blocks for both the free-order session (*m* = 37 %) and the forced-order session (*m* = 34 %). Regarding the results of Experiment 2, I first verified whether participants complied with the given order instruction. Importantly, participants produced larger order reversal rates in random-order blocks during the free-order session (*m* = 34 %) compared with the forced-order session (*m* = 17 %). Concerning the RT measures, I replicated the results of Experiment 1. For task 1, in the free-order session the increase in RTs from fixed-order blocks (*m* = 939 ms) to random-order blocks (*m* = 1119 ms) was smaller (*m* = 180 ms) compared to the increase in RTs from fixed-order blocks (*m* = 909 ms) to random-order blocks (*m* = 1208 ms) in the forced-order session (*m* = 299 ms, see Figure 9). The same holds true for task 2. During the free-order session RT 2 was faster in fixed-order (*m* = 1038 ms) compared with random-order (*m* = 1255 ms) blocks. Also, in the forced-order session RT 2 increased from fixed-order (*m* = 990 ms) to random-order blocks (*m* = 1319 ms). However, in the latter session this increase was larger (*m* = 329 ms) compared to the former session (*m* = 217 ms, a detailed description of the statistical results for both experiments can be found in the original research article in Appendix D).

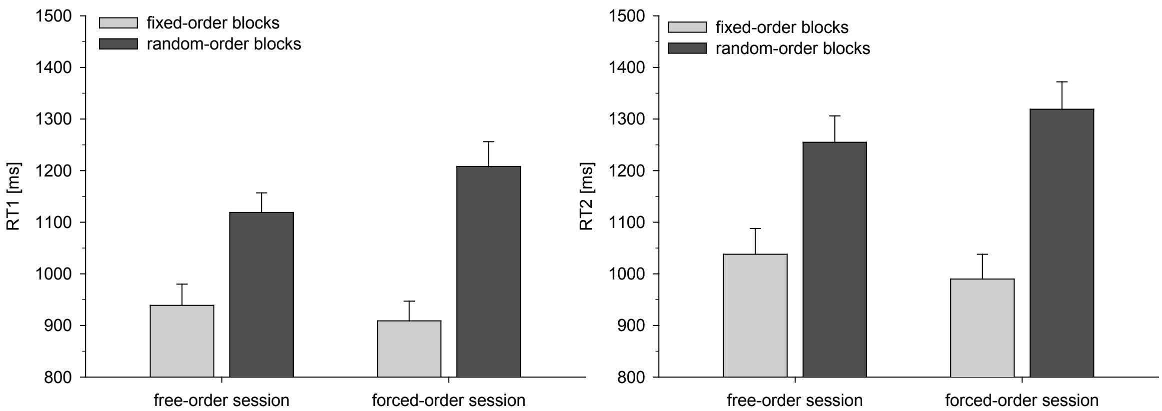

Figure 9: Mean RTs for task 1 and task 2 as a function of block type and session in Experiment 2 of Study 4. Error bars reflect the standard error of the mean. Left panel: RTs for task 1, right panel: RTs for task 2.

To conclude, in Study 4, I demonstrated that task-order coordination processes are affected by task instructions. This was indicated by larger RT increases from fixed-order blocks to random-order blocks under the condition of a forced-order instruction compared with a free-order instruction. These results contradict the assumption that self-organized task scheduling and the implementation of free order choices in dual-task situations result in higher demands on task-order coordination. Instead, I can conclude that adhering to a mandatory order criterion under the forced-order instruction increases demands on task-order coordination processes in comparison to employing an internal order criterion based on free choice under the free-order instructions. This finding confirms earlier assumptions (e.g. De Jong, 1995) suggesting that the requirement to perform a random-order dual task in accordance with the order of stimuli relies heavily on attentional resources. Furthermore, the results of Study 4 are in line with observations from earlier multitasking studies. More specifically, similarly to the findings here, research from the field of task switching has shown that, when participants can freely decide about which task to perform, performance is usually facilitated compared to when task sequences are externally determined (Arrington & Logan, 2005; Gollan et al., 2014). Importantly, the findings of the current experiments go beyond these earlier studies and add important new knowledge to the existing multitasking literature. In particular, I demonstrated that the observation of improved multitasking performance due to an increased degree of freedom is not restricted to the task switching paradigm but can also be found in dual-task situations. To conclude, in Study 4, I demonstrated that, in line with the proposed model, different order criteria affect the employment of task-order coordination processes. More specifically, instructions inflicting mandatory and external order criteria seem to pose higher demands on task-order coordination processes compared to instructions that encourage the employment of internally generated and choice based order criteria. In the General Discussion (Chapter 10), I will further discuss potential mechanisms underlying the effect of different order criteria on task-order coordination.

10. General Discussion

10.1 Summary of the results

The aim of the present dissertation was to investigate the cognitive mechanisms and neural implementation of task-order coordination processes in dual-task situations. To this end, I tested a model of task-order coordination in a series of four studies. For this purpose, in all studies, I applied a dual-task with variable task order resulting in the requirement to schedule the order of task processing. In addition, in Study 3, I applied TMS, a non-invasive brain stimulation technique, to examine the causal relation between task-order coordination and the dlPFC.

In Study 1, I addressed the question of whether the task-order set is processed in working memory. For this purpose, in two studies, I applied a random-order dual-task under low and high working memory load. In Experiment 1, I increased working memory by varying the amount of component task information to be held active in working memory. Importantly, I found that the performance benefit for same-order trials compared with different-order trials was reduced under high relative to low working memory load. In Experiment 2, I replicated these results by implementing a working memory updating task. These findings are in line with the assumption that the task-order set is actively maintained and processed in working memory during dual-task processing. Hence, increasing working memory load during dual tasking hampers the maintenance and processing of the task-order set. The results of Study 1, thus, highlight the role of working memory for task-order coordination.

In Study 2, I investigated whether the task-order set contains order as well as specific task information, or whether both types of information are represented separately. In Experiment 1, I applied a random-order dual task with a variable visual component task. Importantly, I found performance benefits for same-order compared with different-order trials irrespective of a repetition or a change of the visual component task. In Experiment 2, I observed the identical pattern of results for a random-order dual task with a variable auditory component task. These results confirm the assumption that the task-order set only contains information about the sequence of task processing and that specific component task information is represented separately. Also, in Experiment 3, in which I applied two variable auditory and two variable visual tasks, I found evidence for the separate representation of task-order and specific component task information. This suggests that the separate representation of both types of information is not affected by increased demands on maintaining task information active during dual-task processing.

In Study 3, I tested the causal role of the dlPFC for task-order coordination. For this purpose, I applied TMS to the IFJ – a subregion of the dlPFC – during fixed-order and random-order dual-task situations. Importantly, I demonstrated that, when demands on task-order coordination are increased in random-order blocks, TMS of the IFJ prolonged RTs compared to control conditions. In fixed-order blocks, when demands on task-order coordination are reduced, TMS did not affect dual-task performance. This indicates that the dlPFC is indeed causally related to the implementation of task-order coordination processes as has been suggested by previous imaging studies. Furthermore, the causal role for task-order coordination seems to be specific for the dlPFC. This was indicated in Experiment 2, in which I did not find any effects of preSMA TMS on dual-task performance.

In Study 4, I addressed the questions of how different order criteria affect task-order coordination processes. For this purpose, participants received different order instructions during a random-order dual-task situations. In Experiment 1, I used a between-subjects design and showed that the increase in RTs from fixed-order to random-order blocks was higher when participants were instructed to process both tasks according to the order of stimulus presentation, compared to when they could freely decide about their processing sequence. In Experiment 2, I replicated this result in yoked within-subjects design. Together, these findings are in line with the assumption that demands on task-order coordination are increased when participants have to adhere to an external, mandatory order criterion compared to when they can freely decide about their task sequence.

10.2 The role of working memory for task-order coordination

In Study 1, I investigated the role of working memory for task-order coordination processes in dual-task situations. In previous work on task-order coordination, it has been assumed that the task-order set is actively maintained and processed in working memory resulting in performance benefits for same-order compared with different-order trials (e.g. Hirsch et al., 2018; Luria & Meiran, 2006). According to other accounts (e.g. Hommel, 2004; Logan, 2002), however, these performance benefits could also be explained by automatic priming of order information in long-term memory. Importantly, I demonstrated that increasing working memory load resulted in a reduction of the performance benefits for same-order trials. This observation contradicts the assumption of automatic order priming in long-term memory. Instead, in line with the tested model, these findings indicate that the maintenance and processing of the task-order set relies on working memory resources.

This novel finding extends the existing dual-task literature. So far, dual-task research has shown that working memory plays a pivotal role for dual-tasking by making task relevant information, such as information about the component tasks, accessible for cognitive operations. Evidence for this assumption comes, for example, from a study of Ellenbogen and Meiran (2008), who have shown that the amount of interference between two tasks in a dual-task situation is modulated by working memory load. In particular, they demonstrated that interference between tasks (as measured by the backward crosstalk effect) is increased under low working memory load compared to high working memory load. According to the authors, this suggests that when there are sufficient working memory resources available the task sets of the component tasks are maintained and processed concurrently in working memory resulting in interference between both tasks. However, reducing the amount of available resources in a high load condition results in a situation in which only one task at a time can be activated in working memory. As both tasks could not be activated concurrently there was less potential for interference to occur. This notion of active processing of component task information in working memory is further supported by studies on dual-task training. These studies have shown that the extent to which practice effects can occur depends for example on individuals' working memory capacity or the demands on working memory imposed by the applied dual-task situation (Maquestiaux et al., 2004; Schubert & Strobach, 2018; Strobach et al., 2014). Together these findings highlight the role of working memory for maintaining task information, in this specific case component task information, and making it accessible for cognitive operations. Importantly, the present study expands these findings by showing that, in addition to component task information, also task-order information is actively maintained and processed in working memory.

The perspective on task-order coordination as a process that relies on working memory is also supported by broader models on working memory (Oberauer, 2009; 2010; see also Brass et al., 2017; Cowan, 1999). These and similar models propose a prominent role of working memory in goal directed behavior for processing and manipulating task relevant information. For example, in his well-established theory, Oberauer (2009, 2010) proposes that working memory resembles an attentional system that consists of different levels of activation. Furthermore, relevant task representations have to pass through these different levels during task processing. Importantly, the higher the level of activation, the more likely the representation is to gain control over executing cognitive processes and actions. For example, at the lowest level of activation, the *procedural long-term memory*, task representations are only activated at subthreshold level and are not accessible for mental operations. Only on the next level of activation, the *bridge*, task representations reach enough activation in order to guide task processing by being uploaded into the third level of activation, the *response focus*. However, increased activation in higher levels of working memory is accompanied by severe capacity limitations. That is, while the procedural long-term memory has a rather large capacity, only few task representations can be maintained concurrently in the bridge and can gain direct access to the response focus. A consequence of this assumption is that the amount of task information that can be maintained in the bridge depends on the general load the task at hand poses on working memory (Brass et al., 2005). In addition to these storage mechanisms, Oberauer (2009) further proposes additional executive processes that act on the task representations by, for example, manipulating their activation levels or updating them in the bridge.

Importantly, the assumptions on the cognitive mechanisms underlying task-order coordination I tested in Study 1 are applicable to the theory on working memory mechanisms proposed by Oberauer (2009, 2010). Usually, when demands on working memory do not exceed its capacity, the task-order set can be maintained in the bridge together with other task relevant information, such as the task sets of the component tasks. As a result, in same-order trials, the task-order set of the previous trial, which still resides in the bridge, can be easily re-applied and uploaded in the response focus so that only little additional executive processes are necessary. In different-order trials, however, since the task-order set of the previous trial does not specify the correct order on the current trial, additional executive mechanisms need to be employed in order to update or activate a new task-order set in the bridge. This employment of additional executive mechanisms results in increased RTs for different-order compared with same-order trials. In addition, Oberauer's model can also account for the reduction of performance differences between same-order and different-order trials in the high load conditions of Study 1. In particular, due to the increased load, working memory capacity does not suffice to maintain the task-order set active in the bridge throughout an entire dual-task trial. As a result, in same-order trials, participants cannot re-apply the task-order set of the previous trial as it does not reside in the bridge anymore.

Instead, similar to different-order trials, they also have to upload a task-order set into the bridge during same-order trials in the high load condition. Consequently, under the high load condition, processing demands are similar in same-order and different-order trials which resulted in similar performance in both trial types.

An important implication of Study 2 is that task-order coordination processes rely on available working memory resources. Thus, in future studies it should be investigated whether individuals' efficiency to coordinate the sequence of task processing can be predicted by their working memory capacity. For example, it is conceivable that individuals with high working memory capacity, since they might maintain and process task-order information more efficiently, show better performance in dual-task situations with random task order compared with individuals with low working memory capacity. Based on this reasoning, I conducted a study in which I measured performance in fixed-order and random-order blocks (unpublished data). On an individual basis, the difference between these both block types can be used as a measure for the efficiency of task-order coordination processes (Schubert, Fischer, & Stelzel, 2008; Strobach, Hendrich, et al., 2018), with smaller and larger RT differences between blocks indicating more and less efficient task-order coordination, respectively. In addition, I applied a series of complex span tasks (Rummel, Steindorf, Marevic, & Danner, 2019; Unsworth, Heitz, Schrock, & Engle, 2005) in order to assess individuals' working memory capacity. Importantly, and in line with my assumption, I observed a substantial correlation between working memory capacity and the efficiency of task-order coordination processes. More specifically, the better the performance I assessed in the complex span tasks, the smaller was the difference in RTs between fixed-order and random-order blocks. Thus, this study provided preliminary evidence for the assumption that, on an individual level, working memory capacity is predictive for the efficiency of task-order coordination processes. Interestingly, similar assumptions about the role of individual working memory capacity for efficient performance in multitasking situations have been made in the field of task switching. For example, in their recent study, Brüning and Manzey (2018) demonstrated that, on an individual level, working memory capacity is predictive of different processing modes employed during a task switching paradigm. Together, these and other studies suggest that is crucial to further investigate this association between individual working memory capacity and multitasking performance in future studies. Findings from this line of research might be useful for developing tools that could be used to enhance individuals' multitasking efficiency.

10.3 Task-order and component task information is represented separately

In Study 2 of the present dissertation, I investigated which information is represented by the task-order set. Based on the reported findings, I can reject the assumption that the task-order set contains order as well as specific component task information (e.g. Hirsch et al., 2017; Hirsch et al., 2018). On the contrary, in line with the model on task-order organization, I provided evidence for the assumption that the task-order set only contains information about the sequence of task processing; specific component task information is represented separately. The notion of the separate representation of task-order information and component task information has already been hypothesized in earlier studies on task-order coordination (Luria & Meiran, 2003, 2006). Similarly, indirect evidence for this assumption stems from imaging studies that showed that different brain regions are recruited for implementing both types of information (Stelzel et al., 2008). By applying a novel and innovative dual-task paradigm with random task order *and* changing component tasks, I directly tested this assumption and, for the first time, confirmed that task-order and specific component task information is indeed represented separately.

In addition, closer inspection of the data in Experiment 3 of Study 2 suggests that also specific task information for each component task is represented separately by distinct task sets. This is suggested by the observation that the number of component task changes modulates dual-task performance. More specifically, in this experiment, in addition to a random task order, I applied two changing component tasks for each modality, i.e. two auditory and two visual component tasks. This approach yielded trials with two repeated component tasks, trials with one task change, and trials with two task changes relative to the preceding trial. As a result, I found that, irrespective of task order, RTs increased in a stepwise fashion from trials with repeated component tasks ($m_{RT\,1}$ = 989 ms, $m_{RT\,2}$= 1000 ms) over trials with one component task change ($m_{RT\,1}$ = 1036 ms, $m_{RT\,2}$= 1055 ms) to trials with two task changes ($m_{RT\,1}$ = 1078 ms, $m_{RT\,2}$= 1105 ms). Interestingly, this result can be explained by the assumption that the task sets for each component task can be changed individually. In particular, in the condition of one changed component task relative to the preceding trial, only one task set is required to be changed. This is less demanding and results in faster RTs compared to the condition of two changed component tasks. In this case, both task sets have to be changed which poses higher demands and, thus, results in increased processing times. Thus, data from Experiment 3 suggests that not only task-order information is stored separately from specific component task information but also that the particular information for each component task is represented separately by distinct task sets.

Together the findings of Study 2 provide novel insights on the organization of relevant task information during multitasking. In particular, I assume that different types of information, in this case task-order and component task information, are stored separately by distinct representations, e.g. by the task-order set and the particular task sets. This is an important finding as it provides additional insights to the question on how to define a task in multitasking situations (Koch et al., 2018). In particular, multitasking situations are characterized by multiple goals and multiple informational components, e.g. information on different stimuli sets for different tasks or different task rules etc. In order to guarantee appropriate task performance, these different goals and types of information have to be activated simultaneously. An important theoretical question is how these different types of task information are organized or bound together in order to cope with the complex task at hand. The data of Study 2 is in line with the assumption that task information is represented in an agglomerated fashion during multitasking. In other words, different task components are stored by distinct and independent representations, e.g. task sets and task-order sets. During task processing relevant representations are then temporally bundled together into a multi-component agglomeration representing the entire task situation. If necessary, this agglomeration can be flexibly adapted to changing task demands by substituting or replacing individual representational components. Interestingly, such a temporal collection of distinct task components allows for an efficient and economical adjustment to variable environmental demands in multitasking situations.

Further support for the notion of an agglomerated task representation in multitasking also stems from work beyond the dual-task line of research. For example, computational models on cognitive control in multitasking situations assume separate and individually adjustable control parameters for different processing stages, such as response selection or executing motor responses (Logan & Gordon, 2001; Meiran et al., 2008). Also, additional support comes from task switching studies in which not only the tasks themselves but also other task components, such as stimulus dimensions, change from trial to trial (e.g. Kleinsorge & Heuer, 1999). Applying this approach, some studies have shown that different types of information, e.g. stimulus information or information about specific task rules, can be changed and adjusted individually, which confirms the assumption of an agglomerated task representation (Hübner et al., 2001; Rangelov et al., 2013; but see e.g. Philipp & Koch, 2010). Importantly, the results in Study 2 of the present work extend these findings and suggest that the assumption of an agglomerated task representation also holds true in dual-task situations.

An interesting research question for future studies is whether and how the organization of separate representations for task-order and specific component task information can be modified by different task contexts or interventions. For example, in the present study, I employed individual sets of stimuli for each task. This might have biased participants to segregate different types of information and store them in distinct representations. In contrast, using the same set of stimuli for all component tasks may result in a situation, in which task information is represented in an integrated fashion. Additionally, it is also conceivable that training interventions may enable participants to integrate task-order and component task information into a *super representation* that contains the relevant task information all at once. Further research is necessary in order to elucidate the question whether the separate representation of different types of information is a stable and immutable phenomenon across different situations or whether other forms of organizations for different task components are possible under certain circumstances.

10.4 The neural implementation of task-order coordination

In Study 3, I investigated the contribution of the dlPFC for task-order coordination processes. According to the tested model of task-order coordination, this brain region is causally involved in implementing these processes and scheduling the order of task processing. This assumption was based on findings from previous imaging studies that demonstrated increased brain activation in the dlPFC during random-order compared to fixed-order dual tasks (Stelzel et al., 2008; Szameitat et al., 2002). By employing TMS, I confirmed this assumption and demonstrated that the IFJ – a subregion of the dlPFC – is indeed causally involved in task-order coordination providing novel and important insights on the neural underpinning of dual-task processing.

In addition to providing evidence for the causal role of the IFJ, based on the current findings I can also draw conclusions about the functional contribution of this brain structure to task-order coordination. Previous studies suggested that the IFJ is recruited for matching the order of task processing to the order of stimulus presentation in dual-task situations with variable task order (Stelzel et al., 2008; Strobach, Soutschek, et al., 2015). Alternatively, it has been hypothesized that the IFJ is specifically involved in actively changing the order of task processing relative to the preceding trial (Szameitat et al., 2006). Interestingly, while the former process is a general requirement in random-order blocks, i.e. in same-order trials *and* different-order trials, the latter process should only occur in different-order trials. By further subdividing random-order blocks into same-order and different-order trials and investigating the effects of TMS on both trial types, I could test which of these two processes is implemented by the IFJ. Importantly, I found hampered dual-task performance after IFJ TMS in same-order trials as well as different-order trials. Thus, I can exclude that the IFJ is specifically involved in processes exclusively required for different-order trials, such as actively changing task order relative to the preceding trial. If this would be the case, I should have found effects of IFJ TMS only in different-order trials. However, as performance was reduced due to IFJ stimulation in same-order *and* different-order trials, i.e. both trial types of random-order blocks, I can conclude that this brain region is generally recruited for scheduling the processing sequence in dual-task situations with variable task order.

But how, exactly, is this adjustment realized by the IFJ? According to my model of task-order coordination, in random-order but not in fixed-order blocks participants have to monitor the sequence of stimuli and then match their processing order by implementing the appropriate task-order set in working memory. Based on the observation of hampered performance in random-order blocks after stimulation, I propose that the IFJ is directly recruited for the representation and implementation of the task-order set. This assumption is in line with studies investigating the general role of the IFJ for cognitive control processes. Interestingly, in a plethora of studies, this brain region has been found to be recruited for performing tasks that require executive control processes, such as the Stroop task, working memory updating tasks or the task switching paradigm (e.g Derrfuss et al., 2009; Derrfuss et al., 2004; Dove et al., 2000; Muhle-Karbe, Andres, & Brass, 2014). In this context, it has been hypothesized that IFJ serves as a hub that integrates information from adjacent brain regions, such as rather simple S-R information maintained in the premotor cortex and more complex information concerning task goals or abstract task rules stored in more anterior and superior structures of the inferior and middle frontal gyrus (Brass et al., 2005). By doing so, it seems that the IFJ seems to be recruited for adjusting behavior to changing task demands by maintaining and updating complex task information (Derrfuss, Vogt, Fiebach, von Cramon, & Tittgemeyer, 2012). Similarly, according to De Baene, Albers, and Brass (2012), rather than representing *what* to do during task processing, the IFJ seems to specify *how* to perform a specific task in compliance with abstract task rules. Applying this assumption to the dual-task situation used in the present work, it is plausible that the IFJ – rather than representing simple information such as which button to press in response to which specific stimulus – is recruited for representing and instantiating more complex information about the sequence of task processing. In sum, given the current literature on the IFJ, I conclude that the IFJ is involved in implementing executive control processes in dual-task situations with random-task order, namely by maintaining and adjusting task-order information in working memory.

An interesting research question for future studies concerns the functional contribution of other brain regions for performance in dual-task situations with random task order. This is especially important since other structures beyond the IFJ also show increased activation in random-order compared with fixed-order dual-task blocks (Stelzel et al., 2008; Szameitat et al., 2006; Szameitat et al., 2002). These regions include for example the right middle frontal gyrus, dorsal parts of the medial frontal cortex (including the preSMA) as well as the intraparietal sulcus. This suggests that appropriate dual-task performance in random-order dual-task situations is realized by an interplay of different brain region rather than by the IFJ alone. Further studies are necessary in order to disentangle the role and contribution of these other brain regions for the coordination of task processing in dual-task situations.

10.5 The role of different order criteria for task-order coordination

In Study 4, I investigated role of different order criteria on task-order coordination processes in dual-task situations. I demonstrated that instructing participants to respond according to the order of stimulus presentation results in hampered dual-task performance compared with allowing participants to freely decide about their task order. This finding contradicts the assumption that self-organized task scheduling might increase demands on task-order coordination. Instead, the findings of Study 4 clearly indicate that, when participants have to adjust their processing order to the sequence of stimuli and, thus, have to adhere to an external order criterion, demands on task-order are increased compared to dual-task situations with an internally generated and choice based order criterion.

The current findings have important implications for understanding the nature of task-order coordination processes and lead to potential extensions of the knowledge on task scheduling in dual-task situations. However, an important question concerning the present findings remains open. This question concerns the exact mechanism mediating the effect of different order criteria on task-order coordination. An interesting finding that relates to this question is that the requirement to employ task-order coordination processes still seems to occur even under the free-order instructions. In particular, albeit reduced compared to the forced-order instruction, also under the free-order instruction I found a substantial increase in RTs from fixed-order to random-order blocks. Crucially, assuming that task-order coordination would be redundant given a free-order instruction, there should be no difference between fixed-order and random-order blocks. However, as I still found performance differences between both block types, one might conclude that even under a free-order instruction participants employ task-order coordination processes, albeit to a lesser degree. This difference in the amount of employed task-order coordination could be explained by differences in the demands on monitoring related processes. In order to comply with the mandatory criterion under the forced-order instruction, participant have to monitor the order of stimuli and adjust their processing order by selecting the appropriate task-order set. When, in contrast, participants employ a free-order criterion, they can base their processing order entirely on their own order choice. This does not require the monitoring of the stimulus sequence and, hence, reduces demands on task-order coordination. Thus, this explanation highlights the role of monitoring for task-order coordination processes. Furthermore, and equally important, this interpretation suggests that, rather than being employed in an all-or-nothing-fashion, the degree to which task-order coordination is employed during a dual-task situation can be adjusted to variable task requirements such as different order criteria specified by instructions. This view is in line with broader theories suggesting that cognitive control is an adaptable process that is responsive to contextual changes, such as variable task difficulties or previously experienced conflict (e.g. Botvinick, Braver, Barch, Carter, & Cohen, 2001; Verguts & Notebaert, 2009).

Alternatively, an additional mechanism mediating the effect of different order criteria on task-order coordination might be the re-scheduling and optimized sequencing of different sub-processes during dual-task situations. Under a forced-order instruction, task-order coordination processes, i.e. the selection of the appropriate task-order set and its implementation in working memory, can only start after the presentation of the target stimuli. This is so because, due to instruction, the task-order set has to be matched to the sequence of stimuli. Under a free-order instruction, participants do not have to wait for the presented target stimuli because they can select the task-order set based on their free order choice. As a result, task-order coordination processes can start before the presentation of the target stimuli, for example during the inter-trial interval. Thus, the later onset of task-order coordination processes and the additional waiting time under the forced-order compared to the free-order instruction might also explain prolonged RTs. A similar notion on the potential re-scheduling of different sub-processes in dual-task situations comes from training studies. For example, according to Strobach et al. (2014), dual-task practice can result in an optimized scheduling of component task activation and, thus, improved dual-task performance after training. Together, these findings suggest that the sequence of different sub-processes in dual-task situations, e.g. task-order or task-specific processing, is not fixed and irrevocable. Instead, this sequence of processes can be flexibly re-arranged as a response to environmental and internal changes. However, based on the results from Study 4, it is not entirely clear whether the effect of different order criteria on performance is due to an adaptation or a re-scheduling of task-order coordination processes. Additional research is necessary to elucidate this issue.

In future research, it might also be interesting to investigate how other instructions and order criteria influence the employment of task-order coordination processes. Furthermore, in addition to instructions, other factors may also modulate demands on task-order coordination. For example, it is conceivable that increasing the temporal interval between the target stimuli may facilitate performance in dual-task situations with random task order by decreasing the demands on attentional processes that are necessary for monitoring the stimulus sequence. Furthermore, studies on the temporal order judgment task (e.g. Tiippana & Salmela, 2018; Zampini, Shore, & Spence, 2003) have shown that applying target stimuli from the same modality results in better performance compared to applying target stimuli from different modalities (like it is also the case for the dual task applied in the current work). This is probably due to the additional requirement to integrate cross-modal information in the latter situation. In analogy to these results, it might also be easier to adjust the order of task processing to the stimulus sequence in unimodal compared to cross-modal dual-task situations. In the context of usability research, studies investigating these and similar research questions may shed light on the question of how work environments can be adapted to processing limitations in order to guarantee appropriate multitasking performance.

10.6 Future directions

In the present dissertation, I tested a model on active task-coordination processes in dual-task situations. Testing this model shed light on the cognitive and neural mechanisms underlying these processes and expanded the knowledge on cognitive control in dual-task situations. Nevertheless, many open questions require further investigation. For example, the tested model mainly focuses on and specifies active task-order coordination processes. However, according to previous work (De Jong, 1995; Strobach, Hendrich, et al., 2018), an additional mechanism determining task-order in dual-task situations is the "first-come, first-served" principle. Following this assumption, the central bottleneck is passively allocated to both tasks based on central arrival times (Hendrich, 2014; Hendrich et al., 2012; Sigman & Dehaene, 2006). That is, the task that finishes perceptual processing first is also processed first on the bottleneck stage. The other task, which arrives at the bottleneck second, is interrupted passively and has to wait until the other task has left the response selection stage before task processing can proceed. So far both these processes, i.e. the passive "first-come, first-served" principle and active task-order coordination, have only been investigated separately. Thus, an open question relates to whether and how these two mechanisms interact and how both mechanisms conjointly regulate the processing order of two tasks that overlap in time. For example, it is possible that, when there is a larger difference in central arrival times between both tasks, the influence of the task-order set could be smaller due to reduced requirement for top-down control compared to when central arrival times differ to a lesser extent. Alternatively, it is also possible that employing either the "first-come, first-served" principle or active order set based coordination processes is a strategic choice that is affected by, for example, characteristics of the dual-task situation or individual differences. Thus, I propose that future studies should take both active task-order coordination processes and the passive "first-come, first-served" principle into consideration and integrate them into a conclusive model on task order in dual-task situations.

An additional factor that might need further specification in an extended model on task order in dual-task situations concerns the role of the specific component tasks. In particular, so far, the tested model only assumes a one-directional relation between the task-order set and the task sets of the component tasks. That is, the task-order set guides task order by sequentially activating these task sets. However, it is possible that characteristics of the specific component tasks also affect the selection of the task-order set. One of these characteristics might be, for example, the relevance of the component tasks. Research on visual attention has shown that the presentation of relevant versus irrelevant target stimuli decreases visual search times (e.g. Frischen, Eastwood, & Smilek, 2008). Similarly, task relevance can modulate control processes and task selection in the task switching paradigm (e.g. Paulitzki, Risko, Oakman, & Stolz, 2008). Analogously, the increased relevance of one of the component tasks might also affect task order and result in the prioritization of one component task over the other. Coming back to the car driving example from the beginning of this dissertation: Here, executing a breaking response might be far more relevant than maintaining a conversation with the front seat passenger. As a result, processing information that concerns the execution of a breaking response might be prioritized over the processing of other information which is less relevant for driving safety. In addition to task relevance, another factor which might influence the scheduling of multiple tasks concerns the cognitive as well as physical demands the component tasks pose on participants. Evidence for this assumption stems from studies employing multitasking situations with more complex and naturalistic component tasks (e.g. Fournier et al., 2019; Rosenbaum, Gong, & Potts, 2014). Results from these studies suggest that participants use information about potential physical as well as cognitive effort for organizing and planning the sequence of complex actions. Similar phenomena may also occur in dual-task situations (for preliminary evidence, see Leonhard, Fernandez, Ulrich, & Miller, 2011). Other component task characteristics that might also influence task scheduling in dual tasks include the valence of the component tasks (e.g. Johnson, 2009), motivational factors (e.g. Yildiz, Chmielewski, & Beste, 2013) as well as the employed effector systems (e.g Hoffmann, Pieczykolan, Koch, & Huestegge, 2019). Further studies should focus on these and similar task characteristics and investigate whether and how they might influence the scheduling of two tasks that overlap in time.

10.7 Limitations

This work provides novel insights and important theoretical implications for the field of multitasking. Nevertheless, some limitations need further consideration. For example, my methodical approach relied heavily on the PRP framework. In particular, I applied a dual-task paradigm that consisted of two rather simple choice RT tasks. Although this approach made it possible to investigate task-order coordination in a well-controlled and standardized setting, it might entail the risk of oversimplification. That is, the applied dual-task paradigm differs significantly from naturalistic multitasking situations from daily life in various aspects, such as the number and the complexity of the component tasks, their temporal dimensions and many more (Logie, Trawley, & Law, 2011). Thus, it is crucial that future research investigates task-order coordination in a broader framework and tests whether the theoretical implications resulting from this work can also be transferred to more realistic multitasking situations.

Furthermore, a prerequisite of the model tested in this work is the assumption of a capacity limitation at the response selection stage that results in the serial processing of tasks. However, various models on dual-task processing assume that parallel processing is theoretically possible (Logan & Gordon, 2001; Meyer & Kieras, 1997; Tombu & Jolicoeur, 2003). Concerning these and similar models, an important conceptual question concerns whether and how task-order coordination processes can be embedded in frameworks that allow for parallel processing. This is an important question since, at least at first glance, parallel processing might render the occurrence of task-order coordination obsolete. On the other hand, it might be that the processes investigated in this work can also be implemented in these models for similar purposes, for example for regulating the degree of parallel processing and/or capacity sharing (Koch et al., 2018; Tombu & Jolicoeur, 2003). Nevertheless, future research is necessary for elucidating the role of task-order coordination in models allowing for parallel processing.

10.8 Conclusions

To conclude, in the present dissertation I investigated the cognitive mechanisms and neural implementation underlying active task-order coordination processes in dual-task situations. For this purpose, I tested a model which assumes that task-order coordination relies on a representation containing information about the processing sequence of tasks, i.e. the task-order set. I tested this model in a series of four studies applying a dual-task paradigm with random task order. In Study 1, I demonstrated that the scheduling of the component tasks relies on the active processing of the task-order set in working memory. Furthermore, in Study 2, I showed that this task-order set only represents information about the sequence of task processing; specific component task information is stored separately. Additionally, by applying TMS in Study 3, I demonstrated that the dlPFC is causally involved in implementing task-order coordination processes. In Study 4, I provided evidence indicating that instructions can modulate task-order coordination by changing relevant order criteria. These results provide important theoretical implications for multitasking research. Future studies should further expand and specify the tested model of task-order coordination by integrating additional mechanisms and factors that might affect task order in dual-task situations.

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Appendices

Appendix A: Article 1

Appendix B: Article 2

Appendix C: Article 3

Appendix D: Article 4

Appendix E: Eidesstattliche Erklärung

The role of working memory for task-order coordination in dual-task situations

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Abstract

Dual-task (DT) situations require task-order coordination processes that schedule the processing of two temporally overlapping tasks. Theories on task-order coordination suggest that these processes rely on order representations that are actively maintained and processed in working memory (WM). Preliminary evidence for this assumption stems from DT situations with variable task order, where repeating task order relative to the preceding trials results in improved performance compared to changing task order, indicating the processing of task-order information in WM between two succeeding trials. We directly tested this assumption by varying WM load during a DT with variable task order. In Experiment 1, WM load was manipulated by varying the number of stimulus-response mappings of the component tasks. In Experiment 2A, WM load was increased by embedding an additional WM updating task in the applied DT. In both experiments, the performance benefit for trials with repeated relative to trials with changed task order was reduced under high compared to low WM load. These results confirm our assumption that the processing of the task-order information relies on WM resources. In Experiment 2B, we tested whether the results of Experiment 2A can be attributed to introducing an additional task per se rather than to increased WM load by introducing an additional task with low WM load. Importantly, in this experiment, the processing of order information was not affected. In sum, results of the three experiments indicate that task-order coordination relies on order information which is maintained in an accessible state in WM during DT processing.

Keywords: dual tasking, task scheduling, task-order coordination, working memory

Declarations

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Conflict of Interest

The authors declare that they have no conflict of interest.

Ethical approval

Ethical approval: All procedures performed in this study were in accordance with the ethical standards of the institutional and/or national research committee and with the 1964 Helsinki declaration and its later amendments or comparable ethical standards. Approval of the local ethics committee (Humboldt-Universität zu Berlin, Department of Psychology) was obtained before the commencement of the study.

Consent to participate

Informed consent was obtained from all individual participants included in the study.

Consent for publication

All participants gave their informed consent to use their data for publication.

Availability of data and material

The material used and the datasets generated and analyzed during the current study are available from the corresponding author on request.

Introduction

When performing two tasks simultaneously, performance decrements arise compared to situations in which the same tasks are performed in isolation (Koch, Poljac, Müller, & Kiesel, 2018). These dual-task (DT) costs are often explained by limited attention capacity of the cognitive system resulting in a bottleneck during the processing of two temporally overlapping tasks (Pashler, 1994; Pashler & Johnston, 1989; Welford, 1952). According to the central bottleneck account, response selection for both tasks is usually executed sequentially. Therefore, during bottleneck processing of the first task the processing of the second task is interrupted and only continues after response selection for the first task has been completed. Over the last decades, it has been discussed whether this bottleneck constitutes a structural (McCann & Johnston, 1992; Pashler, 1994) or strategic (Fischer & Plessow, 2015; Meyer & Kieras, 1997) limitation of the cognitive system and whether resource allocation to the two tasks can take place in a more gradual and flexible rather than all-or-non fashion (Navon & Miller, 2002; Tombu & Jolicoeur, 2003). However, the question of how the processing order of the tasks is regulated at the bottleneck stage has been mostly neglected.

Different ideas have been proposed about the mechanisms which regulate the processing order in DTs. Earlier studies focused on the idea that task order is passively regulated by the central arrival times of the target stimuli. In that research vein it had been a decisive issue which of two task processing streams finishes perceptual processing first, and thus, reaches the bottleneck before to the other task. Accordingly, this difference in arrival times at the bottleneck determines the processing order in a rather first-come-first-serve principle (De Jong, 1995; Leonhard, Fernandez, Ulrich, & Miller, 2011; Sigman & Dehaene, 2006; Strobach, Hendrich, Kübler, Müller, & Schubert, 2018).

In addition to these earlier accounts, behavioral (Kübler, Reimer, Strobach, & Schubert, 2018; Luria & Meiran, 2003, 2006) and neuronal evidence (Schubert & Szameitat, 2003; Stelzel, Kraft, Brandt, & Schubert, 2008; Töllner, Strobach, Schubert, & Müller, 2012) shows that task order is also coordinated top-down by executive control processes that actively schedule the processing of the component tasks in DT situations. It has been argued that these task-order coordination processes operate on an order control structure that contains information about the processing sequence of tasks and organizes the particular scheduling of the two task streams in each trial (De Jong, 1995; Luria & Meiran, 2003; 2006; see also Hirsch, Nolden, & Koch, 2017). While the role of this order control structure is well established (e.g. Kübler, Soutschek, & Schubert, 2019; Strobach, Soutschek, Antonenko, Floel, & Schubert, 2015), the locus of its processing is still a matter of debate. Some authors assume that the task-order control structure is actively maintained and processed in Working Memory (WM) during DT situations (Luria & Meiran, 2003, 2006). Direct evidence for this assumption, however, is still lacking. Thus, the aim of the current study was to elucidate the role of WM for task-order coordination in DT situations. In particular, we ask whether the maintenance and processing of the order control structure is subject to active WM related processing or whether it is rather subject to a priming-related activation of memory traces form long-term memory. In addition to processing the order control structure, DT situations often require the monitoring of the stimulus sequence. This monitoring is necessary in many DT situations, because participants are usually instructed to process the two tasks according to the order of stimulus presentation (Kübler et al., 2018; Strobach et al., 2018). As an additional question, we will also investigate whether these monitoring related processes also rely on WM resources.

Task-order coordination in DT situations

Evidence for the occurrence of task-order coordination processes stems from DT situations with variable order of the component tasks (De Jong, 1995; Kübler et al., 2018). For example, in a study of Szameitat, Lepsien, von Cramon, Sterr, and Schubert (2006; see also Luria & Meiran, 2003; 2006) the authors administered a DT consisting of an auditory (AUD) and a visual (VIS) choice RT task. The target stimuli of both tasks were presented in quick succession separated by a temporal interval of 200 ms. Furthermore, the sequence of stimulus presentation varied randomly from trial to trial such that either the auditory stimulus was presented first and the visual stimulus second (AUD – VIS trials) or the other way around (VIS – AUD trials). Importantly, participants were instructed to respond to both tasks according to the order of stimulus presentation. As a result, in each DT trial, participants had to monitor the sequence of stimuli and adjust their processing order accordingly imposing the requirement for task-order coordination processes.

For this DT situation, the authors distinguished two types of trials. In same-order trials, the order of both tasks in the current trial n was identical to the order of tasks in the preceding trial n – 1 (e.g., an AUD – VIS trial is preceded by an AUD – VIS trial). In different-order trials, in contrast, the task order in the current trial n was reversed relative to the previous trial n – 1 (e.g., an AUD – VIS trial is preceded by an VIS – AUD trial). When comparing performance between these two trial types, RTs in same-order trials were faster than RTs in different-order trials. According to the authors (see also Kübler et al., 2019; Strobach, Kübler, & Schubert, 2019), this performance benefit for same-order trials indicates the occurrence of task-order coordination processes, which rely on the active processing of task-order information in WM (De Jong, 1995; Luria & Meiran, 2003, 2006; Schubert, 2008; for a similar account on task-pair representations, see also Hirsch, Nolden, & Koch, 2017).

In more detail, Luria and Meiran (2003; 2006) suggested that task order in DT situations is regulated by a higher-order control structure, the task-order set. This task-order set contains information about the processing order of the component tasks and is activated in WM during the processing of a DT trial in addition to the task sets of the component tasks. Here, it guides the order of task processing by sequentially activating the task sets of the component tasks. After its implementation in WM, the task-order set remains active and, thus, affects performance in subsequent trials (see also Hirsch, Nolden, & Koch, 2017; Hirsch, Nolden, Philipp, et al., 2017). In same-order trials, participants can apply the identical task-order set as in the preceding trial. This, in turn, results in faster RTs in same-order trials in comparison to different-order trials. In these different-order trials, a new task-order set has to be instantiated because the task-order set of the preceding trial does not specify the correct order in the current trial. Instantiating a new task-order set is more demanding and takes more time than re-applying the task-order set of the previous trial. This is so because this new task-order set is less activated compared with the task-order set of the previous trial. As a result, RTs in different-order trials are increased compared to RTs in same-order trials. This explanation for RT benefits in same-order compared with different-order trials due to active processing of the task-order set in WM is plausible and also in line with recent accounts on WM and its role for single as well as DT processing (Brass, Liefooghe, Braem, & De Houwer, 2017; Ellenbogen & Meiran, 2008; Oberauer, Souza, Druey, & Gade, 2013; Schubert & Strobach, 2018). Importantly, as WM has only limited capacity, (Baddeley, 2003; Cowan, 2010), this explanation conceptualizes task-order coordination as a resource-dependent process.

Alternatively, rather than active processing in WM, the observation of performance benefits for same-order relative to different-order trials could reflect merely a consequence of automatic priming processes in long-term memory (LTM, Logan, 1988, 2002; Schneider & Logan, 2005; see also Hommel & Eglau, 2002; Mayr & Bryck, 2005; Waszak, Hommel, & Allport, 2003). For example, according to Logan's instance theory (1988, 2002; see also Hommel, 1998, 2004), during task processing task features, such as the order of the processed stimuli or of the processed motor response, are automatically encoded and stored as an integrated episodic trace in LTM. Future events that share features with the stored memory trace can cause its automatic retrieval. This retrieval of memory traces from prior task experience can then facilitate current task performance. Thus, in the context of task scheduling, repeating the task order of the previous trial may activate task-order information in LTM, which then would result in the performance benefits for same-order relative to different-order trials. Importantly, as this explanation relies on rather automatic priming processes in LTM that do not rely on the usage of WM resources, task-order coordination should not rely on the availability of WM resources.

Rationale of the current study

The main goal of the current study was to test, whether task-order coordination relies on the processing and maintenance of a task-order set in WM. To this end, in a series of experiments, we manipulated WM demands during a DT situation with variable task order. The rationale behind this manipulation is the following: WM is characterized by limited capacity (Baddeley, 2003; Cowan, 2010; and other authors). As an example, according to the model of Oberauer (2009; 2010), during task processing, relevant task representations have to be uploaded into an active and accessible state in WM in order to gain control over cognitive operations or actions. However, the amount of information that can be maintained in this state is limited. As a result, overload due to increasing WM demands during a DT situation should hamper the storage of and the access to relevant task information. Applied to the current DT situation, we should find, decreased performance in a high-load compared with a low-load DT situation. Most importantly, we also expect order specific effects of the WM manipulation. If processing the task-order set indeed relies on WM resources during DT situations, then increasing WM demands should specifically hamper the maintenance and processing of the task-order set. As a result, in same-order trials, participants should not be able to make use of the task-order set from the preceding trial because the available WM resources do not suffice to keep the task-order set in an active state. Consequently, the performance benefit for same-order relative to different-order trials should be reduced (or even abolished) in a random-order DT situation with high in comparison to a random-order DT situation with low WM load (Schubert & Strobach, 2018). If, however, the performance benefit for same-order trials can be attributed to automatic priming by stored memory traces in LTM rather than to active processing in WM, then increasing WM load should not impair task-order processing in DT situations. Consequently, we should not find an effect of WM load on the performance benefit for same-order trials compared with different order trials.

In addition to processing the task-order set in WM, a further demand in DTs with variable task order deals with to monitoring related processes: In DT paradigms, participants are usually instructed to respond to both tasks according to the order of stimuli. This does not only require participants to process and instantiate the order set in WM, but also to monitor the stimulus sequence. In fact, monitoring the sequence of stimuli seems to constitute a necessary precondition for implementing the appropriate task-order set in WM (Kübler et al., 2018). Increasing WM load may also disturb these monitoring related processes (instead or in addition to a potential effect on the processing and maintenance of the task order set). To test whether WM load has an effect on such monitoring processes, we administered fixed-order blocks in addition to the random-order blocks in the current experiment. In fixed-order blocks, the task order remains constant throughout a block and, as a consequence, additional monitoring processes are not required in such blocks. Thus, we can compare performance between fixed-order blocks and random-order blocks, which provides us with an indicator for monitoring related processes (Stelzel et al., 2008; Szameitat, Schubert, Muller, & Von Cramon, 2002). In order to test whether the applied WM manipulation affects monitoring related processes, we can then contrast this difference between fixed-order and random-order DT blocks between both load conditions. If monitoring is hampered by increased WM load, we should find increased RT differences between random-order blocks and fixed-order blocks under high compared to low WM load. If monitoring does not rely on WM resources, the WM manipulation should not affect the performance difference between both block types.

We conducted a series of three experiments with different WM manipulations. In Experiment 1, we varied the size of the task sets of the component tasks to be held active in WM by manipulating the number of stimulus-response mappings. In Experiment 2A, we administered a WM updating task in addition to the DT situation to increase the overall WM load of the task situation. In Experiment 2B, we introduced an additional task with low demands on WM in order to test, whether the implementation of an additional task and the need to switch between these tasks or whether increased WM load can be attributed for the results of Experiment 2A.

Experiment 1

In Experiment 1, we manipulated WM demands by varying the size of the task sets of the component tasks (Hick, 1952; Kikumoto & Mayr, 2017; Oberauer, 2009; Schubert & Strobach, 2018; Stelzel et al., 2008). In low-load blocks, participants had to maintain two stimulus-response mappings for each component task in WM, while in high-load blocks, they had to maintain four stimulus-response mappings for each task.

Material and methods

Participants. Twenty-four (21 female) participants aged from 18 to 30 (mean age 22) were recruited from a participant pool at the Institute of Psychology at the Humboldt-Universität in Berlin. Participants were informed about the experimental procedure and gave their consent to participate in the study in advance. As compensation, they received either course credit or 8 euros per hour. Data of one participant were excluded due to high number of erroneous trials (only 55% correct trials).

Stimuli and task. Participants were seated in front of a 24 inch LCD monitor with a 1920 × 1080 pixel resolution and a 144 Hz refresh rate at a viewing distance of 80 cm while performing a DT consisting of an auditory and a visual choice RT task (Stelzel et al., 2008). For the visual task, one of four digits (2, 4, 6, or 8) was presented centrally on a computer screen (.52° x .31°). Responses on the visual stimuli were mapped on the 'M', ',', '.', and '-' buttons of a QWERTZ keyboard in ascending order, and participants were instructed to respond using their right index, middle, ring or little finger, respectively. In the auditory task, participants responded to one of four tones with different pitches (150 Hz, 550 Hz, 950 Hz, or 1350 Hz) presented via headphones by pressing the 'Y', 'X', 'C', and 'V' buttons with their left little, ring, middle and index finger. Participants were instructed to respond to both stimuli as accurately and fast as possible and in the same order they were presented in.

Each DT trial started with a fixation mark that was presented for 750 ms followed by a blank screen for 250 ms. Subsequently, an auditory and a visual stimulus were presented for 200 ms each; stimulus onsets were separated by a time interval of 200 ms. Following the stimuli, the screen was cleared. After responses to both target stimuli or after expiration of a maximal response period of 2750 ms, the next trial began after an inter-trial interval (ITI) of 1250 ms. Error feedback for omitted responses as well as incorrect stimulus discrimination was presented centrally for 500 ms during the ITI and consisted of the German words 'ZU LANGSAM' (too slow) or 'FALSCH' (incorrect). The timing of single-task trials during the practice phase (see below) was similar with the difference that the response period started after the offset of first stimulus and without the presentation of a second stimulus.

Design and Procedure. In total, participants performed twelve blocks with random task order. In these random-order blocks, the order of stimuli varied so that half of the trials were AUD – VIS and the other half VIS – AUD trials. Importantly, the order of stimuli was unpredictable and half of the trials were same-order and the other half different-order trials.

WM load was manipulated by introducing blocks with different numbers of stimulus-response mappings (Stelzel et al., 2008). Throughout high-load blocks, all four visual and all four auditory stimuli were presented as target stimuli, which resulted in eight stimulus-response mappings participant had to maintain active in WM. In low-load blocks, on the contrary, only two stimuli of each task (the digits '4', and '6' for the visual as well as 550 Hz and 950 Hz tones for the auditory task) were presented, which yielded four stimulus-response mappings that had to be maintained in WM. For both, the visual and the auditory task, the intermediate stimuli (differing from each other by the same degree as in high-load blocks) were selected as target stimuli in low-load blocks in order to keep the difficulty of stimulus discrimination constant between both load conditions (Maquestiaux, Laguë-Beauvais, Bherer, & Ruthruff, 2008). Half of the random-order blocks were low-load and the other half were high-load blocks, which resulted in six blocks for each condition. Combining the two factors task order (same-order trials, different-order trials) and WM load (low-load blocks, high-load blocks) in a 2 × 2 design resulted in four DT conditions: Same-order trials and different-order trials from low-load and high-load blocks, respectively. We compared the RT difference between same-order and different-order trials under low and high WM load. In addition to random-order blocks, we presented four fixed-order blocks with high and low WM load. This allowed us to test whether monitoring related processes, which are required in random-order but not ion fixed-order blocks, are affected by increasing WM demands.

Each session started with a practice phase. For each component task, participants performed two single-task blocks with 20 trials each. Half of the participants started with two auditory single-tasks blocks, the other half of the participants started with two visual single-task blocks. For each component task, participants were presented a single-task block with four and a single-task block with eight stimulus-response mappings in counterbalanced order. Then participants performed two random-order blocks à 20 trials for each load condition in counterbalanced order. In the main part of the experiment, participants first performed twelve random-order DT blocks with 33 trials each. Half of the participants first performed six of these random-order blocks under low load and then six blocks under high load. The other half of participants performed these 12 blocks in the reversed order. After finishing the random-order blocks, participants were presented four fixed-order blocks à 48 trials (for an identical sequence of block types, see Kübler et al., 2018), two for each possible task order (AUD – VIS, VIS – AUD). One group of participants first performed these blocks in the low-load condition and then in the high-load condition, whereas for the other group of participants this sequence was reversed. The order of AUD – VIS and VIS – AUD fixed-order blocks was counterbalanced across participants.

Results

We analyzed mean RTs and error rates for both component tasks. For each participant, trials from practice blocks and the first trial of each random-order block were withdrawn from all analyses. For analyses of RTs, erroneous trials (discrimination errors and trials with incorrect task order, *mean*[*m*] = 18 %) as well as trials with RTs slower and faster than +/- 2.5

standard deviation from the mean of each factor combination (*m* = 2 %) were removed from analyses for each participant. RTs and error rates were aggregated across AUD – VIS and VIS – AUD trials. In the first step, we analyzed performance in same-order and different-order trials under low and high WM load using analyses of variance (ANOVAs) with the within-subjects factors WM LOAD (low-load blocks, high-load blocks) and TASK ORDER (same-order trials, different-order trials) separately for the first performed task - task 1 - and the second performed - task 2. In the second step, we analyzed participants' performance in fixed- and random-order block using an ANOVA with the within-subjects factors WM LOAD (low-load blocks, high-load blocks) and BLOCK TYPE (fixed-order blocks, random-order blocks).

Comparison between same-order and different-order trials. As can be seen in Figure 1, RTs for task 1 (RT 1) were significantly increased in high-load blocks (*m* = 1179 ms) compared to low-load blocks (*m* = 989 ms), $F(1, 22) = 101.18$, $p < .001$, $\eta_p^2 = .82$, indicating a general decrement in performance under high WM load. In line with previous research on task-order coordination (De Jong, 1995; Kübler et al., 2018), responses for task 1 were faster in same-order trials (*m* = 1065 ms) than those in different-order trials (*m* = 1103 ms), $F(1, 22) = 15.31$, $p = .001$, $\eta_p^2 = .41$.

Importantly, as indicated by the significant two-way interaction, $F(1, 22) = 17.44$, $p < .001$, $\eta_p^2 = .42$, this performance benefit for same-order compared to different-order trials was modulated by the factor WM LOAD. In low-load blocks, RT 1 was significantly faster in same-order trials (*m* = 955 ms) than in different-order trials (m = 1022 ms), $t(22) = 5.52$, $p < .001$. Contrarily, in high-load blocks, RT 1 in same-order (*m* = 1175 ms) and in different-order trials (*m* = 1183 ms) did not differ significantly, $t(22) = .73$, $p = .48$. Thus, in line with our assumption, increasing WM demands resulted in a reduced performance benefit for same-order versus different-order trials.

Please, insert Figure 1 here

Analyzing accuracy in task 1, we observed more errors in high-load (*m* = 6.1 %) relative to low-load blocks (*m* = 1.9 %), $F(1, 22) = 57.87$, $p < .001$, $\eta_p^2 = .73$ (see Table 1). Overall, mean error rates slightly decreased from same-order trials (*m* = 4.8 %) to different-order trials (*m* = 3.2 %), $F(1, 22) = 11.20$, $p < .01$, $\eta_p^2 = .34$. Additionally, this decrease in errors from same-order to different-order trials was modulated by the factor WM LOAD, $F(1, 22) = 5.83$, $p = .02$, $\eta_p^2 = .21$. While under low load error rates in task 1 did not differ between same-order trials (*m* = 2.2 %) and different-order trials (*m* = 1.6 %), $t(22) = 1.30$, $p = .21$, under high load, we found a performance benefit for different-order trials (*m* = 4.9 %) compared to same-order trials (*m* = 7.3 %), $t(22) = 3.58$, $p = .002$. Thus, although under low load we could not find any performance benefit for same-order trials on the basis of task 1 errors, we found a negative benefit for same-order compared to different-order trials under high load somewhat mirroring the data of RT 1.

For RTs in task 2 (RT 2), a similar pattern of results was identified as compared to RT 1. We observed a reliable main effect of WM LOAD indicating faster RT 2 in low-load blocks (*m* = 1131 ms) compared to high-load blocks (*m* = 1383 ms), $F(1, 22) = 138.87$, $p < .001$, $\eta_p^2 = .86$. Additionally, we found a significant effect of the factor TASK ORDER, $F(1, 22) = 16.41$, $p = .001$, $\eta_p^2 = .43$, reflecting a RT benefit for same-order trials (*m* = 1239 ms) relative to different-order trials (*m* = 1277).

Similar to RT 1, we also found a significant interaction of these two factors, $F(1, 22) = 7.73$, $p = .01$, $\eta_p^2 = .26$. Further analyses revealed that in low-load blocks RT 2 was faster in same-order trials (*m* = 1100 ms) compared to different-order trials (*m* = 1163 ms), $t(22) = 5.36$, $p < .001$. In high-load blocks, the difference between same-order trials (*m* = 1377 ms) and different-order trials (*m* = 1390 ms) did not reach significance, $t(22) = .98$, $p = .34$.

In task 2, participants conducted more errors when WM demands were increased in high-load blocks (*m* = 7.1 %) compared to low-load blocks (*m* = 2.9 %), $F(1, 22) = 76.68$, $p < .001$, $\eta_p^2 = .78$ (Table 1). The main effect of TASK ORDER did not reach significance, $F(1, 22) = 2.01$, $p = .17$, $\eta_p^2 = .08$. The interaction of both factors was significant $F(1, 22) = 5.32$, $p = 03$, $\eta_p^2 = .20$. The non-significant but numerical benefit on the level of task 2 errors for same-order (*m* = 2.6 %) relative to different-order trials (*m* = 3.1 %) under low WM load, $t(22) = .89$, $p = .39$, was reversed in high-load blocks; participants conducted fewer errors in different-order (*m* = 6.1 %) compared to same-order trials (*m* = 8.1 %), $t(22) = 2.20$, $p = .04$. In sum, increasing WM load reduced performance benefits for same-order trials on the level of RT and error rates for task 1 and task 2. This is e in line with the assumption that the task order set cannot be processed efficiently in WM under high load,

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Please, insert Table 1 here

Comparison between fixed-order and random-order blocks. In the next step, we compared RTs and error rates from fixed-order blocks and random-order blocks under both load conditions. This was done in order to test whether increasing WM demands may affect monitoring related processes necessary for DT with variable task order (Kübler et al., 2018). The corresponding ANOVA (all data for the block comparison can be found in Table 2) revealed that RT 1 was increased in high-load blocks (*m* = 1065 ms) compared to low-load blocks (*m* = 863 ms), $F(1, 22) = 177.68$, $p < .001$, $\eta_p^2 = .89$. In addition, we found a reliable effect of the factor BLOCK TYPE, $F(1, 22) = 62.89$, $p < .001$, $\eta_p^2 = .74$, mirrored in increased RT 1 in random-order blocks (*m* = 1084 ms) compared to fixed-order blocks (*m* = 845 ms) and indicating the occurrence of monitoring related processes. Importantly, this increase from fixed-order to random-order blocks was equivalent for both load conditions, as was indicated by the non-significant interaction of the two factors, $F(1, 22) = .54$, $p = .47$, $\eta_p^2 = .02$. Thus, we can conclude that increasing WM demands did not affect monitoring related processes.

When analyzing the error data in task 1, only the factor WM LOAD modulated the frequency of incorrect responses in task 1, $F(1, 22) = 70.88$, $p < .001$, $\eta_p^2 = .76$, with more errors being committed in high-load (*m* = 5.7 %) compared to low-load (*m* = 1.9 %) blocks. Neither the effect of the factor BLOCK TYPE, $F(1, 22) = .46$, $p = .51$, $\eta_p^2 = .02$, nor the interactions of the two factors, $F(1, 22) = 2.12$, $p = .16$, $\eta_p^2 = .09$, was significant.

Also for task 2, increased WM demands in high-load blocks resulted in slowed RTs (*m* = 1269 ms) relative to low-load blocks (*m* = 991 ms), $F(1, 22) = 316.43$, $p < .001$, $\eta_p^2 = .94$. Also, the factor BLOCK TYPE reached significance, $F(1, 22) = 62.89$, $p < .001$, $\eta_p^2 = .74$, with increased RT 2 in random-order (*m* = 1258 ms) relative to fixed-order blocks (*m* = 1003 ms). Similarly to RT 1, this effect of BLOCK TYPE did not differ between both load conditions, as was confirmed by the non-significant interaction of WM LOAD and BLOCK TYPE, $F(1, 22) = 2.63$, $p = .12$, $\eta_p^2 = .11$.

Participants produced more task 2 errors in high-load (*m* = 7.3 %) compared to low-load blocks (*m* = 3.1 %), $F(1, 22) = 67.83$, $p < .001$, $\eta_p^2 = .76$. The effect of the factor BLOCK TYPE, $F(1, 22) = .52$, $p = .48$, $\eta_p^2 = .02$, and the interactions of the two factors, $F(1, 22) = .02$, $p = .88$, $\eta_p^2 < .01$, did not reach significance levels. In sum, these results suggest that increasing WM demands in high-load blocks did not affect monitoring related processes.

This was indicated by similar performance decrements in random-order compared with fixed-order blocks in both load conditions.

Please, insert Table 2 here

Discussion

In Experiment 1, manipulating WM load modulated the performance benefits for same-order relative to different-order trials. In particular, under low load, performance was facilitated in same-order compared with different-order trials replicating earlier findings (Kübler et al., 2018; Luria & Meiran, 2003). In high-load blocks, increasing the number of stimulus-response mappings resulted in a reduction of these performance benefits in task 1 and task 2. These results are in line with the assumption that the processing of the task-order set relies on WM resources and that increasing WM load hampers the processing and maintenance of a task-order set. Furthermore, the current findings are not consistent with the assumption that the performance benefit for same-order trials is due to rather automatic priming in LTM (Logan, 1988; see also Hommel, 1998; 2004). If this was the case, we should have found that WM load does not affect the performance benefits for same-order trials

Additionally, we did not find evidence for the assumption that increasing WM load affects monitoring related processes that are necessary to adjust the task order to the order of stimulus presentation (Kübler et al., 2018; Stelzel et al., 2008). In this case, we should have found larger RT increases from fixed-order to random-order blocks under the high compared to the low-load condition. Instead, we observed similar performance differences between fixed-order and random-order blocks for both load conditions. Thus, these findings do not support the assumption that the monitoring of the stimulus sequence in DTs relies on available WM resources.

While these findings are suggestive for the assumption that the TOC is maintained and processed in WM during DT processing, an important methodological confound needs to be resolved before we can assess the reliability of this conclusion. In more detail, the findings of Experiment 1 could also be explained by a different number of stimulus and response repetitions between high- and low-load conditions. In more detail, we presented two stimuli with two responses and four stimuli with four responses in low-load and high-load blocks, respectively. As a result, there was a higher frequency of stimulus and response repetitions for both tasks in same-order trials of low-load blocks (.5 x .5 = .25) compared to same-order trials of high-load blocks (.25 x .25 ≈ .06). Irrespective of the actual task order, however, repeating stimulus and response features on two succeeding trials may result in performance facilitation (Hommel, Proctor, & Vu, 2004; Mayr, Awh, & Laurey, 2003). Thus, increased numbers of stimulus and response repetitions in low compared to high-load blocks could also explain the results we found in Experiment 1. To address this issue, we conducted Experiment 2A, in which we manipulated WM demands by introducing an additional WM updating task into the DT situation. This allowed us to keep the number of stimulus-response mappings and, thus, the frequency of stimulus and response repetitions constant across load conditions.

Experiment 2A

In Experiment 2A, we implemented an additional WM updating task during a DT with variable task order. In high-load blocks, participants had to maintain a number in WM and, depending on a presented arithmetical stimulus (a '+' sign or a '-' sign), constantly perform an arithmetical task on this number, i.e. count up or down in steps of one. In low-load blocks, we also presented these operators but participants were instructed to simply monitor the sequence of operators. Thus, in addition to the task-order set and the task sets of the component tasks, in both load conditions participants had to maintain additional task information active in WM. However, WM demands were increased in high-load blocks relative to low-load blocks, as participants had to permanently update their WM content, i.e. the result of the ongoing arithmetical task, in high-load blocks (Soutschek, Strobach, & Schubert, 2013), while there was no need to update numerical information in WM during low-load blocks. As in Experiment 1, we assumed that, if the processing of the task-order set indeed relies on WM resources, increasing WM demands should reduce the performance benefit for same-order relative to different-order trials. In addition, and in order to test if monitoring related processes do or do not rely on WM, participants also performed fixed-order blocks under both load conditions.

Material and methods

Participants. Twenty-four (23 female) participants with an age range from 19 to 27 (mean age 22) from the Humboldt-Universität in Berlin and the Martin-Luther University Halle-Wittenberg took part in this experiment. Participants gave their informed consent to participate in the study at the beginning of each session. As compensation, they received either course credit or 8 euros per hour. Data of one participant were excluded due to high number of erroneous trials (only 57% correct trials) and very poor performance in the arithmetical task (an average difference value of 4 for random order blocks, see below).

Apparatus and stimuli. The experimental setting was similar to Experiment 1. Participants performed a DT consisting of an auditory (tone discrimination) and a visual (letter discrimination) component task. For the auditory task, one of three tones (200 Hz, 650 Hz, & 1100 Hz) was presented and participants were asked to respond to these stimuli with their left ring, middle and index finger by pressing the ‘Y’, ‘X’, and ‘C’ buttons, respectively.

In order to not interfere with the arithmetical task, we used a letter discrimination task with the letters ‘A’, ‘E’, and ‘O’ (.52° x .31°) as the visual component task. Participants were instructed to respond to these letters in ascending order by pressing the ‘,’, ‘.’, and ‘-‘ buttons with their right index, middle, and ring finger, respectively. Analogously to Experiment 1, participants were instructed to respond to the target stimuli as fast and as accurately as possible according to the order of their presentation.

Trial timing was adjusted to account for the increased demands posed by the additional WM updating task. At the beginning of each trial, a fixation mark (either a ‘+’ or a ‘-‘ sign) was presented for 1500 ms, which was then followed by both target stimuli. Similar to Experiment 1, each stimulus was presented for 200 ms and the onset of both stimuli was separated by a time interval of 200 ms. Trials ended after participant gave their second response or after a maximum response period of 4000ms. The next trial started after an ITI of 1000 ms, during which feedback for erroneous and omitted responses was given.

Design and Procedure. As in Experiment 1, during random-order blocks, in half of the trials, the auditory stimulus was presented first and in the rest of the trials the visual stimulus was presented first. The order of stimuli varied pseudo-randomly from trial to trial and 50 % of the trials were same-order trials. Block length was shortened in order to reduce the difficulty of the task situation constituted by the combination of the DT and the WM updating task. In total, participants performed 16 random-order blocks consisting of 18 trials.

In both, low-load and high-load blocks, either a ‘+’ sign or a ‘-‘ sign was presented as a fixation mark. The sequence of these operators was randomized throughout each block. In high-load blocks, participants had to keep a number in mind and constantly perform a continuous arithmetical calculation on this number based on the presented operators. Starting from the number ‘10’ at the beginning of each block, participants were instructed to either count up or down in steps of 1 in a continuous fashion. For example, if in the first trial of high-load block a '+' sign was presented, they had to add the number '1' and remember the result ('11'). If in the next trial a '-' sign was presented, they had to retrieve the result from the previous trial ('11') and subtract the number '1' in order to calculate the new result ('10'). Consequently, demands on WM were increased as participants had to constantly maintain and manipulate their WM content. At the end of each block, participants were asked to give their final result of the continuous arithmetical task by writing it on a separate sheet of paper. In low-load blocks, the same arithmetical operators were presented as the fixation mark. However, participants were instructed to solely monitor the sequence of operators throughout the block without performing the additional addition/subtraction task. Consequently, demands on WM were reduced compared to high-load blocks while participants still had to perform an additional task, i.e. the monitoring of the operators.

During a practice phase, participants received 30 single-task trials for each component task in counterbalanced order. Single-task blocks were followed by two random-order DT blocks with 15 trials and with low demands on WM. Afterward, participants received the instructions for the high-load condition and performed three random-order blocks consisting of nine trials under high WM load. In the first part of the main experiment, participants performed 16 random-order blocks consisting of 18 trials, 8 blocks under low load and 8 blocks under high load. The sequence of low and high-load blocks was counterbalanced across participants. After random-order blocks, participants received eight fixed-order blocks with 18 trials each. Half of these blocks consisted of AUD - VIS trials, the other half of VIS - AUD trials. One half of participants first performed these fixed-order blocks in the low-load condition and then in the high-load condition, while the remaining participants performed the low and high load fixed-order blocks in reversed order. The sequence of AUD – VIS and VIS – AUD fixed-order DT blocks was counterbalanced across participants.

Results

Data pre-processing and analysis of DT performance was analogous to Experiment 1. Error trials (*m* = 19 %), as well as trials with RTs slower and faster than +/- 2.5 standard deviation from the mean of each factor combination (*m* = 2 %) were removed before RT analysis for each participant. For analyzing performance in the WM updating task in high-load blocks, we calculated the difference between the correct result and the result given by participants at the end of each block.

Working Memory updating task. Participants exhibited an appropriate accuracy in the WM updating task with an average difference value of *m* = .77 across all blocks. Furthermore, by using a paired sample t-test, we revealed that performance in the WM updating task was impaired in random-order (average difference value of *m* = .96) compared with fixed-order blocks (average difference value of *m* = .47), $t(22) = 3.65$, $p = .001$.

Comparison between same-order and different-order trials. As in Experiment 1, we conducted an ANOVA with the within-subjects factors WM LOAD (low-load blocks, high-load blocks) and TASK ORDER (same-order trials, different-order trials) on RTs and error rates. This analysis demonstrated that RT 1 was significantly slower in high-load blocks (*m* = 1320 ms) than low-load blocks (*m* = 1215 ms), $F(1, 22) = 7.29$, $p = .01$, $\eta_p^2 = .25$. Additionally, RT 1 was reduced in same-order (*m* = 1229 ms) compared to different-order trials (*m* = 1306 ms, see Figure 2), $F(1, 22) = 21.30$, $p < .001$, $\eta_p^2 = .49$, indicating the typical finding of RT benefits for same-order versus different-order trials (De Jong, 1995).

Importantly, we replicated the results of Experiment 1. As indicated by the significant interaction of the two factors TASK ORDER and WM LOAD, $F(1, 22) = 11.28$, $p < .003$, $\eta_p^2 = .34$, the RT benefit for same-order compared to different-order trials was again modulated by the factor WM LOAD. While in low-load blocks RT 1 was significantly faster in same-order trials (m = 1157 ms) compared to different-order trials (m = 1272 ms), $t(22) = 6.95$, $p < .001$, no such benefit for same- (m = 1302 ms) versus different-order trials (m = 1339 ms) could be found in high-load blocks, $t(22) = 1.58$, $p = .13$. Thus, for RT 1, high compared to low WM demands yielded a reduced performance benefit for same-order trials.

Please, insert Figure 2 here

For errors in task 1, the only significant effect was found for the factor WM LOAD, $F(1, 22) = 12.06$, $p = .02$, $\eta_p^2 = .35$, indicating that errors in task 1 occurred more often in high-load (m = 4.0 %) compared to low-load blocks (m = 2.5 %, see Table 1). Neither the main effect of TASK ORDER, $F(1, 22) = 2.06$, $p = .17$, $\eta_p^2 = .09$, nor the interaction of the two factors, $F(1, 22) = .69$, $p = .41$, $\eta_p^2 = .30$ reached significance.

Also for RT 2, we found a significant main effect for the factor WM LOAD, $F(1, 22) = 7.03$, $p = .015$, $\eta_p^2 = .24$, with slower responses for the high-load (m = 1498 ms) compared to the low-load condition (m = 1372 ms). Additionally, we found a significant main effect for the factor TASK ORDER, $F(1, 22) = 15.20$, $p = .001$, $\eta_p^2 = .41$, indicating an RT benefit for same-order trials (m = 1404 ms) in contrast to different-order trials (m = 1466 ms).

Furthermore, the performance benefit in RT 2 for same-order relative to different-order trials differed between load conditions, $F(1, 22) = 12.88$, $p = .002$, $\eta_p^2 = .37$. The significant performance benefit for same-order trials (*m* = 1319 ms) compared to different-order trials (*m* = 1425 ms) in low-load blocks, $t(22) = 6.63$, $p < .001$, could not be replicated in high-load blocks. Instead, in the latter block type, RT 2 did not differ significantly between same-order (*m* = 1490 ms) and different-order trials (*m* = 1507 ms), $t(22) = .70$, $p = .49$.

For errors in task 2 (see Table 1) no effect was significant, with $F(1, 22) = .95$, $p = .34$, $\eta_p^2 = .04$ for the factor WM LOAD, with $F(1, 22) = .31$, $p = .58$, $\eta_p^2 = .01$ for the factor TASK ORDER, and with $F(1, 22) = .97$, $p = .34$, $\eta_p^2 = .04$ for the interaction of these two factors. In sum, analyses of RTs replicated the findings of Experiment 1, consistent with our assumption that the task-order set cannot be processed efficiently in WM when WM demands are high.

Comparison between fixed-order and random-order blocks. In addition, we separately analyzed RT 1 and RT 2 and error rates using an ANOVA with the within-subjects factors WM LOAD (low-load blocks, high-load blocks) and BLOCK TYPE (fixed-order blocks, random-order blocks). In comparison to the low-load condition (*m* = 1050 ms), RT 1 was increased in the high-load condition (*m* = 1184 ms), $F(1, 22) = 26.22$, $p < .001$, $\eta_p^2 = .54$. Additionally, responses on task 1 were slower in random-order blocks (*m* = 1267 ms) relative to fixed-order blocks (*m* = 966 ms), $F(1, 22) = 65.55$, $p < .001$, $\eta_p^2 = .75$. Importantly, this increase from fixed-order to random-order blocks was similar for both load conditions, as was indicated by the non-significant interaction of these two factors, $F(1, 22) = 1.91$, $p = .18$.

Regarding accuracy in task 1, participant produced more errors when WM demands were high (*m* = 3.2 %) compared to when they were low (*m* = 1.7 %), $F(1, 22) = 20.65$, $p < .001$, $\eta_p^2 = .48$. Also, more errors could be observed in random-order (*m* = 3.2 %) relative to fixed-order blocks (*m* = 1.7 %), $F(1, 22) = 37.98$, $p < .001$, $\eta_p^2 = .63$. The interaction between the two factors was not significant, $F(1, 22) = .28$, $p = .60$, $\eta_p^2 = .63$.

For RT 2 we found similar results: RTs in high-load blocks (*m* = 1357 ms) were slower than RTs in low-load blocks (*m* = 1201 ms), $F(1, 22) = 22.65$, $p < .001$, $\eta_p^2 = .51$. Additionally, RT 2 was increased in random- (*m* = 1435 ms) compared fixed-order blocks (*m* = 1233 ms), $F(1, 22) = 60.48$, $p < .001$, $\eta_p^2 = .73$. This increase in RT 2 from fixed-order to random-order blocks was similar in the low-load and high-load condition, as was indicated by the non-significant interaction of the two factors, $F(1, 22) = 1.60$, $p = .22$, $\eta_p^2 = .07$.

For error rates in task 2, no significant effect of the factors WM LOAD, $F(1, 22) = 2.65$, $p = .12$, $\eta_p^2 = .11$, BLOCK TYPE, $F(1, 22) = 1.92$, $p = .18$, $\eta_p^2 = .08$, nor for their interaction, $F(1, 22) = 1.43$, $p = .71$, $\eta_p^2 = .01$, could be observed. In sum, RT and error data suggest that monitoring related processes were, again, not affected by the WM manipulation. This was indicated by similar performance decrements in random-order relative to fixed-order blocks under low and high WM load. Data of the block comparison can be found in Table 2.

Discussion

In Experiment 2A, we replicated the findings of Experiment 1: When WM load was low, we found a performance benefit in task 1 and task 2 for same-order compared to different-order trials (Luria & Meiran, 2003, 2006). Increasing load due to the additional WM updating task resulted in absent performance benefits for same-order trials. Again, these results are in line with the assumption that the processing of the task-order set requires WM resources and that increasing WM load hampers the processing and maintenance of a task-order set. Importantly, in Experiment 2A, we manipulated WM demands by applying a WM updating task during DT performance. Thus, we can exclude the alternative explanation for the results of Experiment 1, according to which the decreased performance benefit for same-order trials could be accounted for by an increased number of stimulus-response repetitions in low-load compared to high-load blocks (Hommel et al., 2004; Mayr et al., 2003). Also, by observing similar performance decrements in random-order compared to fixed-order blocks under both load conditions, we found no evidence for the modulation of monitoring related processes due to increased WM demands (Stelzel et al., 2008).

However, we need to address a potential alternative explanation for the results of Experiment 2A in an additional Experiment 2B. More specifically, in this Experiment, we wanted to test whether switching between a DT situation and an additional task per se (i.e. irrespective of WM demands) can decrease the performance benefits for same-order relative to different-order trials. In high-load blocks of Experiment 2A, we enforced participants to perform the additional arithmetical task and prompted them to give their final result at the end of each block. In low load-blocks, in contrast, participants were instructed to simply monitor the operators and no overt response was required. Thus, there was no control regarding participants' performance in the low-load condition. Consequently, in low-load blocks, we cannot exclude that participants might have performed the DT and avoided the additional task, i.e. monitoring the operators. This might, at least theoretically, be problematic, because then in high-load blocks, participants had to switch between a DT and a highly demanding WM updating task, whereas in low-load blocks participants may have performed only the DT without switching between the two different tasks. As a result, both conditions might not only differ in WM demands but also in the additional requirement to switch between a DT and an additional task. Importantly, this additional switching rather than increased WM may be responsible for reduced performance benefits in same-order relative to different-order trials in high-load blocks of Experiment 2A.

To elucidate whether the switching between the DT and an additional task alone can evoke the disappearance of the performance benefits for same-order trial, in Experiment 2B we incorporated a Go/NoGo task (Donders, 1969) in random-order DT blocks. We included this Go/NoGo as the additional task because it resembles the monitoring task from low-load blocks of Experiment 2A: First, the Go/NoGo task is characterized by low demands on WM. Second, to perform this task correctly, participants have to monitor the sequence of stimuli from trial to trial (and decide whether to press a button or not) which was also the core requirement for the monitoring task in low-load blocks of Experiment 2A. Furthermore, the Go/NoGo task requires an overt response (at least in some trials). Consequently, we can verify whether participants also performed the additional task or whether they only focused on the DT without performing this additional Go/NoGo task.

Experiment 2B

The aim of Experiment 2B was to investigate, whether the switching between a random-order DT and an additional task with low WM demands alone can result in decreased performance benefits for same-order relative to different-order trials. For this purpose, we used a Go/NoGo task (Donders, 1969). Similar to the additional task in low load blocks of Experiment 2A, this Go/NoGo requires participants to monitor the sequence of stimuli while keeping demands on WM rather low. Importantly, if switching between a random-order DT and an additional Go/NoGo alone results in the disappearance of the performance benefit for same-order trials, we should find no RT difference between same-order compared with different-order trials. On the other hand, if we still find faster RTs in same-order trials despite the additional Go/NoGo task, we can conclude that switching between a DT and an additional task alone is not sufficient to reduce the performance benefit for same-order trial.

Material and Methods

Participants. 24 participants (17 female, mean age 22 years) from the Humboldt-Universität in Berlin, who gave their informed consent in advance, took part in Experiment 2B. As compensation, they received either course credit or 8 euros per hour.

Design and Procedure. Participants performed 12 random-order blocks from Experiment 2A. However, instead of performing an additional arithmetical or monitoring task, participants were instructed to perform a Go/NoGo task (Donders, 1969) upon the fixation mark. For this purpose, participants were asked to respond to the '-'-sign by pressing the space-button with their (either left or right) thumb while withholding their response in case of the '+' sign. By applying this Go/NoGo with an overt response, we guaranteed that participants attended to the additional task while keeping WM demands to a minimum.

Results

Analysis of the Go/NoGo task indicated appropriate performance with a mean error rate of 3.86 % (SD = 5.49 %) for omission errors (misses) and .31 % (SD = .66 %) for commission errors (false alarms). To test for a performance benefit for same-order compared to different-order trials despite the additional Go/NoGo task, RTs and error rates (separately for task 1 and task 2) were analyzed. Data preprocessing and aggregation was equivalent compared to the previous experiments. Only trials with correct responses in both tasks (m = 73 %) were included and trials within a range of +/- 2.5 standard deviation from the mean of each factor combination (m = 2 %) were excluded from RT analyses.

Comparison between same-order and different-order trials. To test for better performance in same-order trial versus different-order trials, we analyzed RT 1 using paired simple t-tests. Importantly, despite the additional Go/NoGo task, this analysis revealed faster responses in same-order (m = 1136 ms) compared to different-order trials (m = 1227 ms),

$t(23) = 3.27$, $p = .003$[1]. Similarly, error rates in task 1 increased from same-order trials (m = 4.2 %) to different-order trials (m = 6.3 %), $t(23) = 2.98$, $p = .007$.

A similar result was found for RT 2 with a RT benefit for same-order (m = 1354 ms) compared with different-order trials (m = 1426 ms), $t(23) = 2.63$, $p = .015$[2]. Error rates in task 2 were not affected by a change in task order, $t(23) = .91$, $p = .37$. Thus, RT data from task 1 and task 2 demonstrated a performance benefit for same-order relative to different-order trials also when introducing an additional Go/NoGo task with low demands on WM.

Comparison across Experiment 2A and 2B. In Experiment 2B we demonstrated faster RTs for same-order compared to different-order trials despite introducing a Go/NoGo task into a DT situation with variable task order. To further confirm this benefit for same-order trials despite a Go/NoGo task with low demands on WM, we compared RT data of Experiment 2B with the RT data from random-order blocks in the low load condition of

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[1] A supplementary ANOVA with the factor TASK ORDER (same-order trial, different-order trial) and the additional factor Go/NoGo (go trials, no go trials) demonstrated that the performance benefit for same-order compared to different-order trials was equal for go-trials and no-go trials in task 1. This was indicated by the non-significant effect of the interaction between TASK ORDER an Go/NoGO, $F(1, 23) = 1.30$, $p = .27$, $\eta_p^2 = .05$.

[2] A supplementary ANOVA with the factor TASK ORDER (same-order trial, different-order trial) and the additional factor Go/NoGo (go trials, no go trials) demonstrated that the performance benefit for same-order compared to different-order trials was equal for go-trials and no-go trials in task 2. This was indicated by the non-significant effect of the interaction between TASK ORDER an Go/NoGO, $F(1, 23) = .70$, $p = .41$, $\eta_p^2 = .03$.

Experiment 2A. Note that demands on WM should be similar in low-load blocks from Experiment 2A, in which participants had to monitor the operands presented at the beginning of each trial, and in Experiment 2B, in which participant had to monitor the operands *and* give a response whenever a “-“ sign was presented as the fixation mark. As a result, we expected similar performance benefits for same- compared to different-order trials in both situations.

To test this assumption, we performed an ANOVA with the within-subjects factor TASK ORDER (same-order trials, different-order trials) and the between-subjects factor EXPERIMENT (Experiment 2A, Experiment 2B) on RTs and error rates in task 1 and task 2. For RTs in task 1, we found a significant effect of the factor TASK ORDER, $F(1, 45) = 39.82$, $p < .001$, $\eta_p^2 = .47$, indicating faster RT 1 in same-order (m = 1146 ms) compared to different-order trials (m = 1249 ms). Importantly, this performance benefit for same-order trials was equal in the low-load condition of Experiment 2A and in Experiment 2B; the combination of TASK ORDER and EXPERIMENT was non-significant, $F(1, 45) = .57$, $p = .45$, $\eta_p^2 = .01$. The factor EXPERIMENT did not reach significance, $F(1, 45) = 1.00$, $p = .76$, $\eta_p^2 < .01$. Also, when analyzing accuracy data for task 1, we could not find any evidence that the difference in error rates between same-order and different-order trials varied across Experiment 2A and Experiment 2B. This was indicated by the non-significant interaction of the factors TASK ORDER and EXPERIMENT, $F(1, 45) = .98$, $p = .33$, $\eta_p^2 = .02$. Furthermore, the factor ORDER reached significance, $F(1, 45) = 13.18$, $p = .001$, $\eta_p^2 = .23$, indicating increased error rates in task 1 for different-order (m = 4.4 %) relative to same-order trials (m = 2.8 %) across both experiment. The effect of the factor EXPERIMENT, $F(1, 45) = 9.04$, $p < .01$, $\eta_p^2 = .17$, indicating slightly increased error rates in task 1 in Experiment 2B (m = 4.9 %) compared to error rates in Experiment 2A (m = 2.4 %).

We found a similar pattern for task 2. RT 2 was significantly faster in same-order trials (m = 1336 ms) relative to different-order trials (m = 1423 ms), $F(1, 45) = 30.81$, $p < .001$, $\eta_p^2 < .41$. Importantly, this performance benefit was similar in both experiments, as the interaction of the factors TASK ORDER and EXPERIMENT was not significant, $F(1, 45) = 1.14$, $p = .29$, $\eta_p^2 = .03$. Furthermore, the factor EXPERIMENT was not significant, $F(1, 45) = .03$, $p = .87$, $\eta_p^2 < .01$. For error rates in task 2, neither the factors ORDER, $F(1, 45) = .01$, $p = .91$, $\eta_p^2 < .01$, EXPERIMENT, $F(1, 45) = 1.70$, $p = .20$, $\eta_p^2 = .04$, nor their interaction, $F(1, 45) = 1.66$, $p = .20$, $\eta_p^2 = .04$, were significant. Thus, comparing RT and error rate with data from low-load blocks of Experiment 2A further confirms that switching between an additional Go/NoGo task (with low demands on WM) and a random-order DT did not affect the performance benefits for same-order compared with different-order trials in Experiment 2B

Discussion

In Experiment 2B, we demonstrated that also in face of an additional Go/NoGo task with low demands on WM, performance is improved in same-order compared to different-order trials. These performance benefits were similar to those found in the low-load condition in Experiment 2A as was confirmed by an additional comparison between both experiments. Thus, the mere demand to switch between two different tasks, i.e. the random-order DT and an additional task, does not lead to the reduction of performance benefits for same-order relative to different-order trials. Therefore, with respect to the findings of Experiment 2A, we conclude that the disappearance of the performance benefit for same-order compared to different-order trials cannot merely be explained by the need to switch between the random-order DT and the WM updating task. Instead these results can most likely be attributed to increased WM demands in high-load blocks of Experiment 2A.

General Discussion

The aim of the present study was to investigate the role of WM for task-order coordination in DT situations. For this purpose, in the first two experiments, we introduced a WM manipulation during a DT with variable order of the component tasks. In both experiments, in low-load conditions, we found a performance benefit for same-order trials compared to different-order trials (De Jong, 1995; Luria & Meiran, 2003, 2006; Szameitat et al., 2006). In contrast, when WM load was increased, this performance benefit vanished and no difference in RTs could be observed between same-order and different-order trials. This result confirms the assumption that the processing of the task-order set relies on WM resources (Luria & Meiran, 2003, 2006; Szameitat et al., 2006). As a result, increasing WM load hampers this processing of the task order set and the benefit for same-order versus different-order trials is reduced in high compared with low WM load conditions. In Experiment 1, this was shown by varying the number of stimulus-response mappings for each task (Stelzel et al., 2008). In Experiment 2A, we replicated the results of Experiment 1 by introducing an additional WM updating task (Soutschek et al., 2013). This was necessary, as the reduced performance benefits for same-order trials in low-load compared to high-load blocks in Experiment 1 could also be explained by different frequencies of stimulus and response repetitions (Hommel et al., 2004; Mayr et al., 2003). Furthermore, in Experiment 2B, we tested whether switching between a random-order DT and an additional task alone (rather than increased WM load) causes the disappearance of the performance benefit for same-order relative to different-order trials. Importantly, in this experiment, we still found performance benefits for same-order trials compared with different-order trials despite an additional Go/NoGo task (Donders, 1969) with low demands on WM. Thus, we conclude that the findings of Experiment 2A, can most likely not be attributed to switching between two different tasks alone but rather to increased WM demands. In sum, the results of the current study do not support the assumption that the performance benefit for same-order trials occurs due to automatic priming in LTM (e.g. Logan, 1988). Instead, they are consistent with the assumption that the task-order set is actively maintained and processed in WM during DT processing and, thus highlight the role of WM for task-order coordination.

In addition, we also investigated, whether WM load affects the monitoring of the stimulus sequence, which is necessary due to the instruction to respond to the tasks according to the order of stimulus presentation. To this aim, we compared the performance decrements between fixed-order blocks, in which the stimuli were presented in fixed order and monitoring was not necessary, and random-order blocks (Stelzel et al., 2008). In Experiment 1 and Experiment 2A, we demonstrated that increasing WM load did not affect monitoring related processes in DT with variable task order. This was indicated by similar RT increases from fixed-order to random-order blocks in low-load and high-load blocks.

The Role of WM for DT Situations

There is ample evidence suggesting that performing more than one task simultaneously draws on WM resources, which are necessary to represent relevant task information and make this information accessible for various cognitive operations and actions (Law, Trawley, Brown, Stephens, & Logie, 2013; McDowell, Whyte, & D'Esposito, 1997; Redick et al., 2016; Todorov, Kubik, Carelli, Del Missier, & Mäntylä, 2018). So far, however, in the field of DT research, this view has been largely limited to the level of the component tasks. More specifically, it has been argued that specific task information of both component tasks, i.e. the task sets, have to be maintained in an active state in WM during DT processing (Huestegge & Koch, 2010; Oberauer, 2009; Schubert & Strobach, 2018).

Direct evidence for the fact that the task sets of both tasks are actively maintained and processed in WM stems for example from a study by Ellenbogen and Meiran (2008). In this study, the authors investigated the modulatory effect of WM load on the backward crosstalk effect. The backward crosstalk effect occurs due to code overlap between both tasks and refers to the phenomenon that responses in task 1 are facilitated in compatible trials, in which stimulus and/or response dimensions of task 1 overlap with those of task 2, compared with incompatible trials, in which there is no overlap between tasks (Hommel, 1998; Miller, 2006). In their study, Ellenbogen and Meiran (2008) manipulated WM load blockwise by varying the number of stimulus-response mappings in the first task. By doing so, in a series of experiments, they found that the reliable backward crosstalk effect under low WM load was reduced to non-significant levels when WM load was increased. According to the authors, this data pattern suggests that, as long as there is sufficient WM capacity, both task sets are activated concurrently in WM resulting in interference between tasks and, thus, the backward crosstalk effect. However, increasing the number of stimulus-response mappings for task 1 caused WM overload and only one task set could be instantiated in WM. As a consequence, there was no possibility for interference to occur and, accordingly, the backward crosstalk effect was reduced to zero.

Similar evidence for the role of WM for DT processing stems from training studies, which demonstrate that training gains in DT performance depend on individuals' WM capacity as well as WM demands imposed by the practiced DT (Maquestiaux et al., 2004; Schubert & Strobach, 2018; Strobach, Salminen, Karbach, & Schubert, 2014). For instance, in a study of Maquestiaux, Hartley, and Bertsch (2004), the authors compared the reduction of DT costs after training of a complex DT situation (with high demands on WM) between younger and older adults. They demonstrated that, while DT interference was considerably reduced in younger adults, older adults only slightly benefited from DT training. According to the authors, this pattern of results suggests that, in the course of training, younger adults learned to simultaneously instantiate the complex task sets of the component tasks in WM. For older adults, on the other hand, the complexity of the task sets imposed higher demands on their WM capacity. As a result, they could not capitalize as much on the training regimen as younger adults did resulting in smaller training gains. However, when reducing WM demands imposed by task 1 (Experiment 2) or task 2 (Experiment 3), older adults showed a similar reduction in DT interference after training compared to younger adults. In other words, after adjusting task demands to their WM capacity, older participants were also able to concurrently instantiate the task sets of the component task in WM due to training. This shows that WM plays a crucial role for DT performance by maintaining relevant information of the component tasks in an active state and, thus, guarantees the instantiation of tasks sets of two temporally overlapping tasks.

The findings of the current experiments go beyond these earlier studies and add important new knowledge to the existing DT literature. More specifically, the current results indicate that not only specific information of the component tasks, i.e. the tasks sets, is maintained and processed in WM, but also higher-order information about the processing sequence of tasks. On a theoretical level, it has been argued that a task-order set, containing information of the specific task order in a given trial, is processed in WM during DTs with variable task order (Luria & Meiran, 2003, 2006; Schubert, 2008). So far, preliminary evidence for this assumption stems from the fact that RTs are faster when task order is repeated (in same-order trials) compared to when task order changes (in different-order trials) relative to the previous trials. This finding suggests that the task-order set of the preceding trial is still active in WM after it has been applied (De Jong, 1995; Kübler et al., 2018; Luria

& Meiran, 2003). The findings of the current study demonstrate that the performance benefit for same-order compared to different-order trials vanishes when pushing WM capacity to its limits. This indicates that the task-order set cannot be processed efficiently in WM between two succeeding trials when load is increased. Thus, the current results confirm, first, that sequence information about the to be processed tasks is hold and processed in WM in addition to the task sets of the component tasks and, second, that factors influencing the efficiency of WM affect task order processing in DT situations.

The data of the present study are in line with current models that propose a prominent role of WM for task processing (Cowan, 1999; Oberauer, 2009; 2010; for a suggestion on the neural implementation of WM see also Brass et al., 2017). For example, the model of Oberauer (2009, 2010) conceptualizes WM as an attentional system that selects relevant task representations, e.g. task sets, and makes them accessible in order to guarantee goal-directed behavior. For this purpose, the model proposes that task representations sequentially pass through different levels of activation during task processing. Importantly, the higher the level of activation, the more susceptible they are to capacity limitations. In the *activated part of procedural long-term memory*, procedural representations are activated at subthreshold level. This component of WM has a rather large capacity, however, representations cannot gain direct control over cognitive operations or actions. For this purpose, representations have to enter the second level of activation, the *bridge*. The *bridge* holds task information and task sets that are "currently in control of thought and action" (Oberauer, 2009, p. 58) and makes them directly accessible for operation in the third level, the *response focus*. Increased activation in and access to implementation in the response focus, however, go along with a limited capacity of the bridge. As a result, only a restricted amount of information can be maintained at this level. Consequently, the amount of task information which can be maintained active in the bridge depends on the load it imposes on WM with simple tasks allowing all information to be transferred concurrently into the bridge, whereas with increasing load only partial task information can be maintained (Brass et al., 2017). The current findings suggest that not only information of the specific component tasks but also higher order information about the processing order of tasks is represented in WM. In line with this assumption, in low-load conditions of the current experiments, participants were able to upload the task-order set as well as the component task sets into the bridge, resulting in faster RTs in the subsequent trial, when task order was repeated, i.e. in same-order trials. In contrast, when load was high, either due to the increased number of stimulus-response mappings (Experiment 1) or additional task demands (Experiment 2A), the task-order set could not be maintained in the bridge during the entire course of a DT trial. As a result, in the subsequent same-order trial, the task-order set had to be reloaded into the bridge, resulting in absent performance benefits compared with different-order trials. Thus, based on the results of this study, we can conclude that not only specific task information but also information about the sequence or order several tasks is processed and maintained in WM during DTs.

Monitoring related processes and WM

While increasing WM load does affect the storage and processing of the task-order set, the current WM manipulation did not affect monitoring related processes that are required to perform both tasks according to the order of stimulus presentation. This is line with results from a study of Stelzel et al. (2008) in which the authors employed functional magnetic resonance imaging. In this study, the authors compared fixed-order and random-order blocks under low and high WM load by using a similar manipulation as we did in Experiment 1. As a result, they demonstrated that monitoring related processes, mirrored by contrasting fixed-order and random-order blocks, and WM related processes, reflected by contrasting low-load and high-load blocks, rely on dissociable brain structures. More specifically, increasing WM load was associated with increased brain activation in caudal parts of the premotor cortex and the anterior insula. Monitoring, on the other hand, was correlated with increased activation in more anterior parts of the prefrontal cortex surrounding the inferior frontal sulcus. According to Stelzel et al. (2008), this result pattern suggests that monitoring and maintenance of task-order information are dissociable processes that are implemented by different brain structures.

Further support for the fact that monitoring and task-order set processing are indeed distinct mechanisms comes from a study of Kübler et al. (2018). In this study, the authors applied a random-order DT and varied the task-order instruction participants had to adhere to during DT processing. While in one condition participants were instructed to respond to both tasks according to the order of stimulus presentation, in the other they could freely decide in which order to perform the tasks. As a result, in the former condition participants had to employ monitoring related processes, in order to adjust their processing order to the stimulus sequence, whereas in the latter condition there was no need to monitor the order of stimuli presentation. As a result, the performance difference between fixed-order and random-order blocks was reduced when participants could freely decide about task order compared with when they had to match task order to the stimulus sequence. Kübler et al. (2018) concluded this result indicates that changing the instruction in DT situations can affect monitoring related processes. In contrast, the difference between same-order and different-order trials, which reflects the processing of the task-order set in WM, did not differ between both conditions and, thus, was not affected by the instruction manipulation. According to the authors, this dissociation indicates that the performance difference between fixed-order and random-order blocks on the one, and the performance difference between same-order and different-order trials on the other hand might reflect independent mechanisms of task-order coordination, i.e. the monitoring related processes and the processing of the task-order set in WM. The results of the present study confirm the assumptions of these previous studies. More specifically, by demonstrating in two experiments that varying WM load does affect the efficient employment and processing of the task-order set in WM but not the monitoring of the stimulus sequence, we provide further evidence for the fact that monitoring and processing of order information in WM are dissociable processes that both are necessary for scheduling the sequence of task processing in DT situations.

Conclusion

In the present study we investigated the role of WM for task-order coordination in DT situations. For this purpose, in a series of experiments, we varied WM load during DT blocks with variable task-order. We demonstrated that increasing WM load results in reduced performance benefits for same-order trials relative to different-order trials. This confirms our assumption that task-order information cannot be maintained in an accessible state when WM capacity is at its limits and, thus, highlights the role of WM for task-order coordination.

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Table Caption

Table 1: Error rates for task 1 and task 2 in % (and standard deviation) in same-order and different-order trials as a function of Working Memory (WM) load for Experiment 1 and Experiment 2.

Table 2: Mean reaction times (RTs) in ms (and standard deviation) and error rates in % for task 1 and task 2 in fixed-order and random-order blocks as a function of Working Memory (WM) load for Experiment 1 and Experiment 2.

Table 1:

	Experiment 1			
	WM load			
	low		high	
	same-order trial	different-order trial	same-order trial	different-order trial
error rate task 1	2.2 (2.6)	1.6 (2.2)	7.3 (3.2)	4.9 (3.2)
error rate task 2	2.6 (1.7)	3.1 (2.6)	8.1 (3.7)	6.1 (3.5
	Experiment 2			
	WM load			
	low		low	
	same-order trial	different-order trial	same-order trial	different-order trial
error rate task 1	1.8 (1.9)	3.0 (2.2)	3.8 (2.8)	4.2 (4.0)
error rate task 2	5.0 (3.4)	4.2 (3.7)	5.0 (3.5)	5.2 (3.5)

Table 2:

	Experiment 1			
	WM load			
	low		high	
	fixed-order block	random-order block	fixed-order block	random-order block
RT 1	738 (204)	989 (282)	951 (211)	1179 (276)
RT 2	851 (230)	1131 (296)	1155 (226)	1384 (264)
error rate task 1	1.9 (2.7)	1.9 (2.1)	5.3 (3.4)	6.1 (2.8)
error rate task 2	3.2 (3.1)	2.9 (1.7)	7.4 (4.2)	7.1 (2.8)
	Experiment 2			
	WM load			
	low		low	
	fixed-order block	random-order block	fixed-order block	random-order block
RT 1	885 (325)	1215 (409)	1047 (355)	1319 (438)
RT 2	1031 (323)	1372 (407)	1215 (378)	1498 (465)
error rate task 1	1.0 (1.5)	2.4 (1.6)	2.4 (2.2)	4.0 (2.8)
error rate task 2	3.5 (3.1)	4.6 (2.9)	4.5 (3.7)	5.1 (3.2)

Figure Caption

Figure 1: Mean RTs for task 1 and task 2 as a function of trial type and Working Memory load for Experiment 1. Error bars reflect the standard error of the mean. Asterisks indicate significant differences between same-order and different-order trials conditions. Left panel: reaction times for task 1 (RT 1), right panel: reaction times for task 2 (RT 2).

Figure 2: Mean RTs for task 1 and task 2 as a function of trial type and Working Memory load for Experiment 1. Error bars reflect the standard error of the mean. Asterisks indicate significant differences between same-order and different-order trials conditions. Left panel: reaction times for task 1 (RT 1), right panel: reaction times for task 2 (RT 2).

Figure 1:

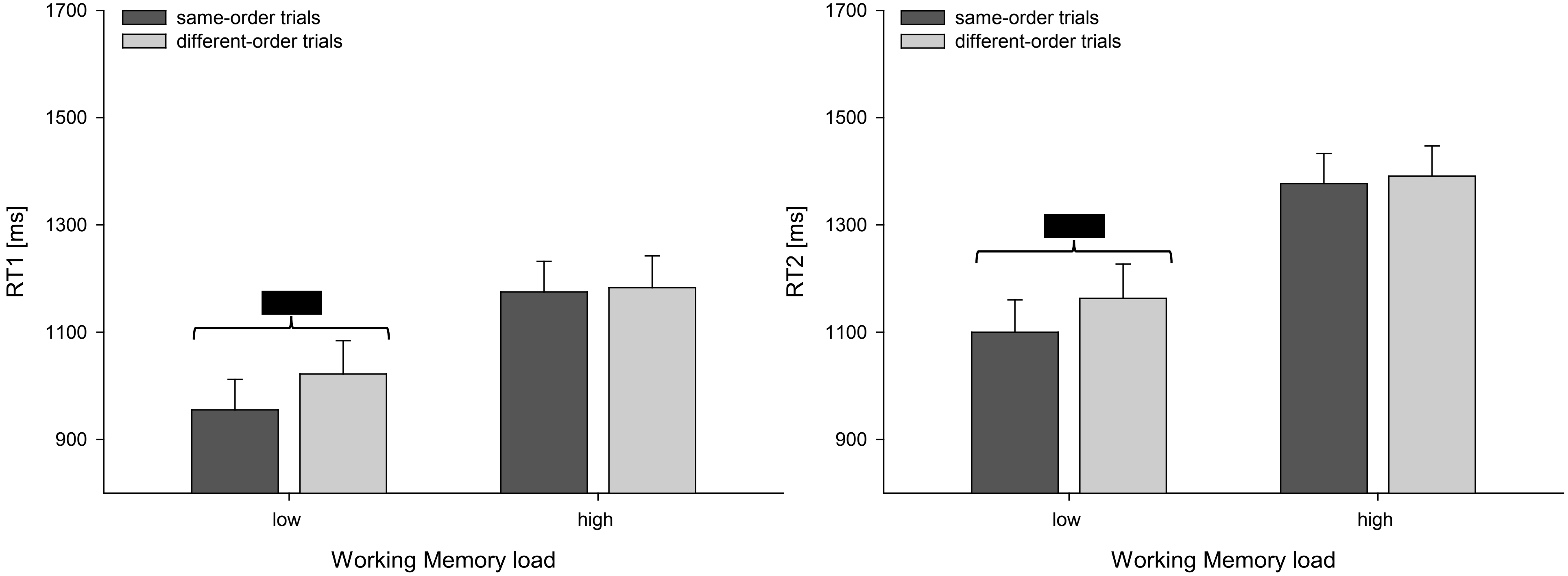
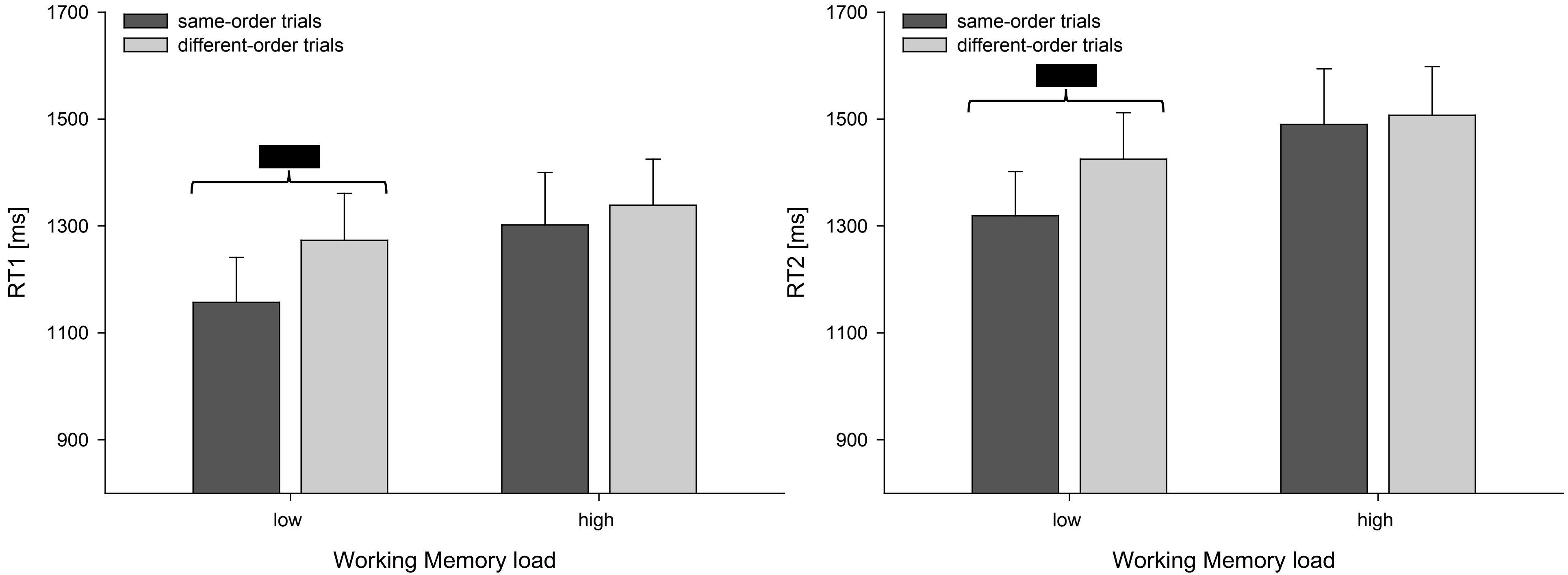

1700
1500
1300
1100
900
RT1 [ms]
same-order trials
different-order trials
low
high
Working Memory load
1700
1500
1300
1100
900
RT2 [ms]
same-order trials
different-order trials
low
high
Working Memory load

Figure 2:


1700
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RT1 [ms]
same-order trials
different-order trials
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1100
900
RT2 [ms]
same-order trials
different-order trials
[redacted]
low
high
Working Memory load

On the Organization of Order and Task-Specific Information in Dual-Tasks Situations

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Abstract

Dual-tasks (DT) require the employment of task-order representations that schedule the processing of two tasks. Evidence for this assumption stems from the observation that in DTs with variable order, performance is improved in trials with repeated processing order relative to the preceding trial in comparison to trials with reversed processing order. So far, it is an open question whether these order representations only contain order information or whether they also integrate component task information. To tackle this question, we applied a DT with variable task order consisting of an auditory and a visual task. In Experiment 1, in addition to task order, the visual task varied randomly from trial to trial while the auditory task kept constant. In Experiment 2, the auditory task varied. In Experiment 3, both component tasks varied. In all experiments, performance benefits occurred in trials with a repeated relative to trials with a reversed processing order, irrespective of a repeated or a changed component task. This indicates that order representations in DTs only contain order information. The findings are in line with the view that multitasking situations are represented as an agglomeration of distinct components that can be individually adjusted to changing task demands.

Keywords:

dual-task situations, task order coordination, PRP paradigm, task switching, task sets, executive control,

Public Significance Statements

During multitasking, i.e. the performance of multiple tasks at the same time, different types of information have to be processed. By applying a specific version of a dual-task paradigm, the present study investigates how these different types of information, i.e. task-order information and specific component task-information, are mentally organized. The findings strongly suggest that different types of information are represented separately by distinct representations. This is in line with the view, that multitasking situations can be divided in different informational task components. These components are conjointly represented in an aggregated form and can be adjusted individually on the spot if required.

Introduction

Performing two (or more) tasks simultaneously often comes at a cost compared to performing only one task at a time. In the laboratory, this can be shown by applying a variety of different dual-task (DT) paradigms, such as the paradigm of the Psychological Refractory Period (PRP) (Pashler, 1994; Pashler & Johnston, 1989; Welford, 1952). In this PRP paradigm, participants perform two temporally overlapping choice reaction time (RT) tasks. As a typical finding for PRP-like and other DT situations, RTs and/or error rates are usually increased compared to situations in which the same tasks are performed in isolation. These DT costs are often attributed to a capacity limitation, i.e. a bottleneck, at the response selection stage. This bottleneck usually processes only one task at a time and, thus, causes the serial processing of the two tasks. So far, research has focused on whether this bottleneck is provoked by a structural (e.g. Pashler, 1994; Schubert, 1999) or a rather strategic (e.g. Fischer & Plessow, 2015; Meyer & Kieras, 1997) limitation, or whether the bottleneck can be subjected to the flexible allocation of attentional resources (e.g. Navon & Miller, 2002; Tombu & Jolicoeur, 2003). However, an important issue, which has hardly been addressed so far, concerns the scheduling of both tasks at the bottleneck: Since response selection is usually executed only for one task at a time, competition arises between the two tasks for access to the bottleneck.

In order to resolve this competition, there is the requirement for regulating the processing order of both tasks and temporally coordinating their processing streams along the bottleneck (Stelzel, Kraft, Brandt, & Schubert, 2008). On the one hand, the processing order of both tasks can be determined bottom up by the central arrival times of the component tasks, i.e. the task that finishes perceptional processing first enters the bottleneck first and bottleneck processing of the other task is postponed (De Jong, 1995; Sigman & Dehaene, 2006; Strobach, Hendrich, Kübler, Müller, & Schubert, 2018). In addition to this first-come-first-serve principle, on the other hand, there is now a growing body of evidence from behavioral

(Kübler, Reimer, Strobach, & Schubert, 2018; Lien & Ruthruff, 2004; Luria & Meiran, 2003, 2006) as well as psychophysiological studies (Kübler, Soutschek, & Schubert, 2019; Szameitat, Lepsien, von Cramon, Sterr, & Schubert, 2006; Szameitat, Schubert, Muller, & Von Cramon, 2002) indicating that task order can also be regulated top down. This top-down regulation is controlled by order representations that contain information about the processing sequence of the component tasks. So far, however, it is unclear how task-order information is specified by these task-order representations. Thus, the goal of the present study was to investigate the content and organization of these task-order representations and develop a more detailed model of a mental task representation in DT situations.

Evidence for Task-Order Representations in DT Situations

Evidence for the existence of task-order representations guiding the sequence of task processing comes from a specific adaptation of the PRP paradigm (Szameitat et al., 2006; see also De Jong, 1995, Luria & Meiran, 2003, 2006). For instance, in the study of Szameitat and colleagues (2006), the authors applied two temporally overlapping component tasks, i.e. a tone discrimination task (Task A) and location discrimination task (Task B). They presented the target stimuli of these component tasks with variable order and asked participants to respond to both tasks according to the order of stimulus presentation. In more detail, the target stimuli were presented with a randomly changing stimulus onset asynchrony (SOA) of +/-200 ms; a positive SOA indicating that task A was presented as the first and task B as the second task, and a negative SOA indicating the reversed order. As a result, the target stimuli of the component tasks were presented either in the order AB or BA, and the order of stimulus presentation varied randomly from trial to trial (e.g. AB – AB – BA – AB – BA – BA – …, with the two letters A and B indicating the stimuli of the component tasks and the hyphen indicating a break between two trials). Due to instruction, in this DT situation, participants had to constantly adjust their processing order to the variable order of stimuli.

The authors investigated the effect of task-order changes on DT performance. For this purpose, they distinguished two trial types. For *same order trials*, the task order on a given trial *n* (e.g. AB) was the same as in the previous trial *n – 1* (AB – AB). For *different-order trials*, on the contrary, the task order on a given trial *n* was reversed relative to the preceding trial *n – 1* (BA – AB). As a result, the authors showed that RTs for both component tasks were faster in same-order trials compared to RTs in different-order trials. Additionally, participants produced less task-order reversals in same-order trials compared to different-order trials. Note that in these order reversal trials, participants responded to the target stimuli in the reversed order relative to the order they were presented in. Thus, in these trials, participants did not adhere to the given order instruction and produced a scheduling error.

This pattern of results is interpreted with the assumption of a task-order representation, the task-order set, that contains information about the processing sequence of both component tasks (Szameitat et al., 2006). During the course of the current DT trial *n*, this order set is activated and then guides task order processing by allocating the central bottleneck to the task that has to be performed first and reallocating it to the second task after response selection for the first task has finished (Schubert, 2008; see also De Jong, 1995; Luria & Meiran, 2003, 2006). If the task order is repeated on two consecutive trials, as it is the case for same-order trials, participants can re-apply the order set of the previous trial *n – 1* because it can still be used to appropriately guide the processing order of the two component tasks in trial *n*. This results in an RT advantage in both tasks for same-order trials compared with different-order trials. In different-order trials, the order of tasks in the current trial *n* is reversed relative to the order of the component tasks in the preceding trial *n - 1*. As a result, and in order to avoid a task-order reversal, participants need to overcome the task-order set indicating the task order of the previous trial and need to implement a new task-order set. This is accompanied by increased processing demands and results in slowed RTs in different-order compared with same-order trials (Szameitat et al., 2006). In addition to RTs, increased task-order reversal rates in different-order relative to same-order trials indicate the failure of implementing a new task-order set. More specifically, in these cases, participants could not overcome the inappropriate order set of the previous trial *n – 1* and, instead, re-apply it in the current trial *n*. As a consequence, participants respond to both tasks in the order of the preceding trial resulting in a task order that is reversed compared to the stimulus order of the current trial. Thus, task-order reversal rates are increased in different- compared to same-order trials. In sum, the observation of performance benefits for same-order compared with different-order trials, indicates the operation of a task-order representation during DTs.

The Organization of the Task-Order Set

The occurrence of RT benefits for same-order versus different-order trials due to the operation of the task-order set has been replicated in numerous studies (Kübler et al., 2018; Steinhauser & Steinhauser, 2018; Strobach, Antonenko, et al., 2018) and has also been causally related to the lateral prefrontal cortex (Stelzel et al., 2008; Strobach, Soutschek, Antonenko, Floel, & Schubert, 2015; Szameitat et al., 2006). Although it seems to be a stable and reliable phenomenon, basic features of the task-order set have not been investigated yet. More specifically, it is still an open question which information relevant for the DT at hand is exactly specified by the task-order set. In addition to the particular order information, participants have to maintain and implement the specific task information, i.e. the task sets, of the component tasks during DT processing (Ellenbogen & Meiran, 2008; Strobach & Schubert, 2017). In contrast to the order set, these task sets do not represent order but instead specific task information, such as stimulus and response information and the stimulus-response (S-R) mappings. An important question is, how these different elements or components of information, i.e. order information as well as specific task information, are organized in a mental task representation of a DT. Theoretically, two different assumptions about the content of the task-order set are conceivable. Importantly, these two accounts make different predictions about the type and organization of information specified by the order set.

The *separated task-order set hypothesis* proposes that the task-order set only represents the sequence of task processing but not the specific component tasks. Thus, task-order and task specific information is represented separately by the task-order set and task sets of the component tasks, respectively. For illustration, imagine a DT with variable task order consisting of an auditory tone discrimination task, in which participants respond to tones with different pitches by pressing response buttons with their left hand, and a visual letter discrimination task, in which participants respond to different letters by pressing response buttons with their right hand. According to the separated order set hypothesis, the order set only contains information about the processing order of both component tasks, such as to perform the auditory task first and then the visual task second. Importantly, it would not further specify the stimuli (i.e. different tones and different letters), the responses and the S-R mappings of the respective tasks. In other words, the order set only indicates the sequence of task processing (e.g. perform the auditory task first and the visual task second) but it *does not* represent specific component task information and, thus, *does not* indicate which button to press as a response to specific target stimuli (e.g. respond to the low pitch tone by pressing the left response button with your left middle finger and respond to the letter *K* by pressing the right response button with your right index finger).This specific task information is represented separately by the task sets of the component tasks. Thus, task-order and specific task information is stored separately by distinct representations

This separated task-order set hypothesis, thus, suggests that task-order and specific task information is represented in an agglomerated fashion, i.e. different informational components are specified by different representational elements that are more or less independent. During DT processing these representations can then be flexibly assembled and individually changed in order to cope with the task at hand. Indirect evidence for this assumption, for example, stems from a neuroimaging study of Stelzel et al. (2008) who demonstrated that task-order and component task information is stored in and implemented by distinct brain regions. Similarly, evidence for separate components constituting the representation of a task stems from computational models of cognitive control in multitasking situations (Logan & Gordon, 2001; Meiran, Kessler, & Adi-Japha, 2008) and from studies in the field of task switching (Hübner, Futterer, & Steinhauser, 2001; Rangelov, Töllner, Mueller, & Zehetleitner, 2013).

Alternatively, and according to the *integrated task-order set hypothesis*, the task-order set integrates information about task order as well as information about the component tasks constituting the DT situation. Thus, the order set does not only specify which task to perform first and which task to perform second, but it also specifies the particular stimuli, the responses, and the S-R mappings for each component task. For the current example of a DT task consisting of a letter and a tone task, this would mean that the task-order set, on the one hand, would indicate the order of task processing *and*, on the other hand, it would *also* specify the information necessary to cope with the two component tasks at hand (e.g. respond first to the low pitch tone by pressing the left response button with your left middle finger and second to the letter *K* by pressing the right response button with your right index finger). In other words, according to the integrated order set hypothesis, the task-order set incorporates the information stored in the task sets of the component tasks and forms an integrated task representation, which combines order and task specific information.

Rationale of the Current Study

In order to investigate whether the separated task-order or the integrated task-order set hypothesis is appropriate, we employed a new DT paradigm, in which not only task order but also the specific component tasks varied randomly from trial to trial. Recent DT studies employing DTs with fixed order but randomly changing component tasks have shown that either changing the first (Hirsch, Nolden, & Koch, 2017) or the second component task (Hirsch, Nolden, Philipp, & Koch, 2017) relative to the preceding trial results in increased RTs compared to trials in which both component tasks are repeated. According to the authors, this pattern of results can be explained with the assumption that participants activate specific task information, i.e. the task sets of the component tasks, before performing a DT trial. When repeating both component tasks from the previous trial, participants can perform the current trial by re-employing the task sets of the previous trial resulting in relatively fast RTs. However, when changing one of the component tasks relative to the preceding trial, participants cannot re-employ the same task sets of the preceding trial. Instead they need to change the task set of one of the two component tasks, which imposes additional processing demands resulting in increased RTs relative to trials with repeated component tasks.

Crucially, combining both approaches and introducing two independently changing task components, i.e. applying a DT with variable task order (see Kübler et al., 2018; Luria & Meiran, 2003, 2006) *and* randomly changing component tasks (see Hirsch, Nolden, & Koch, 2017; Hirsch, Nolden, Philipp, et al., 2017), allowed us to disentangle the organization of task-order and component task information. Note that a similar approach of independently changing task components has been applied in the field of task switching in order to investigate the organization of single task representations (Kleinsorge, 2004; Kleinsorge & Heuer, 1999; Philipp & Koch, 2010; Rangelov, Töllner, Mueller, & Zehetleitner, 2013; Vandierendonck, Christiaens, & Liefooghe, 2008).

To put the separated and integrated task-order set hypotheses to a test, we examined the performance benefit for same-order trials compared with different-order trials under two conditions: A condition of two repeated component tasks and a condition of one (Experiment 1 & Experiment 2) or two (Experiment 3) changed component tasks relative to the previous trial. Importantly, both hypotheses make different predictions about the performance benefit for same-order relative to different-order trials for the case that a component task has changed between consecutive DT trials.

According to the separated task-order set hypothesis, the task-order set only specifies the processing order of both tasks. Task-specific information, on the other hand, is represented separately by the task-sets of the component tasks. Therefore, in same-order trials, participants should be able to re-apply the task-order set of the preceding trial irrespective of a repetition or a change of the component tasks. Even if the specific component task changes, participants can still use the task-order set of the preceding trial. This is so, because the order set would only contain information about the task sequence and changing one component task (while repeating task order) does not require the instantiation of a new task-order set. As a result, if the separated task-order set hypothesis is true, we expect an RT benefit for same-order trials compared with different-order trials for situations with a repeated *and* a changed component task. Similar to RTs, also task-order reversal rates should be lower in same-order compared to different-order trials irrespective of a repetition or a change of the component tasks. Increased task-order reversal rates in different-order trials indicate that participants have failed to overcome the task-order set of the previous trial. While, even after a changed component task, participants can still re-apply task-order set of the previous trial in same-order trials, in different-order trials they have to overcome this task-order set. Consequently, even under the condition of a changed component task, we should find decreased task-order reversal rates for same-order relative to different-order trials. In sum, according to the separated order set hypothesis, we should find performance benefits for same-order compared with different-order trials irrespective of a repetition or a change of the component tasks.

According to the integrated task-order set hypothesis, the task-order set represents complex information that specifies not only the processing order but also the specific component tasks in a DT trial. Similar to the separated order set hypothesis, for the case of repeated component tasks, we should find a performance benefit for same-order trials compared with different-order trials as participants can re-apply the task-order set of the previous trial. In contrast to the separated order set hypothesis, however, there should be no performance difference between same-order and different-order trials after the change of a component task. This is so because, according to the integrated order set hypothesis, the order set contains information about the processing order *and* about the particular component tasks in an integrated form. Thus, for the case of a situation with a changed component task, in same-order trials, the task-order set of the preceding trial cannot be re-applied anymore because it does not fit the changed component task. Therefore, a new task-order set, which includes the correct information of the new component task has to be activated despite the fact that task order is repeated. Similarly, for different-order trials with a changed component task, a new task-order set has to be activated because the task-order set of the preceding trial does not fit neither the particular task order nor the particular component task of the current trial. Thus, the integrated task-order set hypothesis predicts that, after the change of a component task, a new task-order set has to be activated for both same-order and different-order trials in order to meet the new task composition of the current trial. Therefore, there should be no RT benefit for same-order compared with different-order trials in case of a changed component task. The same prediction holds for task-order reversal rates. Also here, there should be no difference between same-order and different-order trials in situations with changed component tasks. In this situation, in both same-order and different-order trials the task-order set of the previous trial has to be overcome since the activated task-order set does not match the task composition anymore. Thus, there is now the same chance for implementing an incorrect task-order set for both types of trials. All in all, according to the integrated task-order set hypothesis, there should be no performance difference between same-order and different-order trials when one of the component tasks has been changed relative to the preceding trial. In other words, we should only find a performance benefit for trials in which *both* task order *and* the component tasks repeat.

We tested both predictions in three experiments. In all experiments the stimulus order of the two component tasks, an auditory and a visual choice RT task, varied randomly and unpredictably from trial to trial and participants were asked to respond to the tasks in the order of stimulus presentation. The experiments differed with respect to the changing component task; while in Experiment 1, the visual component task changed randomly from trial to trial, the auditory component task changed in the same manner in Experiment 2. In Experiment 3, both component tasks changed randomly from trial to trial.

Experiment 1

In Experiment 1, participants performed a DT consisting of an auditory and one of two visual component tasks (either a letter or a digit discrimination task). Importantly, and in order to put the separated and integrated task-order set hypothesis to a test, task order varied *and* the specific visual component could change randomly from trial to trial. According to the separated task-order set hypothesis, we should find shorter RTs in same-order compared with different order trials in the condition of a repeated visual component task and we should find shorter RTs in same-order compared with different order trials also in the condition of a changed visual component task. In other words, the separated task-order set hypothesis predicts performance benefits for same-order versus different-order trials irrespective of a repetition or change of the visual component task. In contrast, the integrated task-order set hypothesis predicts no performance benefit for same-order trials compared to different order trials in the condition of a changed visual component task.

Materials and Method

Participants Twenty participants (13 female) from the Martin-Luther Universität Halle-Wittenberg, with a mean age of 20.8 years (*SD* = 5.0 years), took part in Experiment 1. This sample size was based on an a priori power analysis using the G*Power software (Faul, Erdfelder, Lang, & Buchner, 2007), which allows for testing effect power of main effects in repeated measures designs with two factors consisting of two factor levels each (2 × 2 designs). For this end, we estimated a large effect size of $\eta_p^2 = .29$ (which corresponds to an effect size of $f = .63$) based on the results of Kübler et al. (2019, Experiment 1). That study showed an effect of task order on DT performance by comparing conditions with variable and fixed task order. The remaining parameters for the power analysis were specified as follows: Test family: F tests; Statistical test: ANOVA: Repeated measures, within factors; Type of power analysis: a priori; α err prob: .05; Power (1 - β err prob): .95; Number of groups: 1; Number of measurements: 2; Corr. among rep measures: .00 (note that reducing the correlation between repeated measures to zero provides a rather conservative estimation of the required sample size, as increases in this correlation result in higher power and, thus, smaller required sample sizes); Nonsphericity correction ε: 1. This analysis yielded a required sample size of N = 19. We increased this required sample size to 20 in order to compensate for potential dropout due to low accuracy or similar.

All participants had normal or corrected-to-normal vision and were right-handed. They were not aware of the purpose of the present study and were paid 8 euros per hour or course credit for their participation. Approval of the local ethics committee was obtained before the start of the study and written informed consent was collected from each participant. For all experiments, an inclusion criterion of at least 60 % of correct trials was applied. Data of one participant were excluded due to low accuracy (44.4 % correct trials).

Apparatus and tasks Participants sat in front of a 24 inch LCD monitor with a resolution of 1920 × 1080 pixels and a refresh rate of 144 Hz. Timed stimulus presentation and data acquisition were controlled by the Presentation software (Version 18.0 12.05.14). Auditory stimuli consisted of one of two sine wave tones with a low (200 HZ) or a high pitch (1100 Hz) presented via headphones. Participants were asked to respond to the low and high pitch tone by pressing the 'Y' and 'X' key of a QWERTZ keyboard with their left middle and index finger, respectively. For the *digit task*, either the digit '3' or the digit '7', and, for the *letter task*, the letter 'C' or 'M' was presented in white centrally on a black background. All visual stimuli subtended approximately .52° x .31° of the visual angle at a viewing distance of 80 cm. In both visual tasks, participants were asked to perform a choice RT task on the target stimuli. Responses for the *letter* and the *digit task* were mapped on the same response keys: Participants were instructed to press the ',' key in response to the digit '3' and the letter 'C', and the '.' key in response to the digit '7' and the letter 'M' with their right index and middle fingers, respectively. Participants were instructed to respond to both stimuli as fast and as accurately as possible and, importantly, according to the order of their presentation

Design and procedure DT trials started with the presentation of a fixation cross for 1000 ms after which an auditory and a visual stimulus were presented sequentially for 200 ms each. Stimuli were separated by an SOA +/- 200 ms with a positive SOA indicating that the auditory stimulus was presented as the first and the visual task as the second stimulus, and a negative SOA indicating the reversed order (Figure 1). After stimulus presentation, the screen was cleared for the response period, which was interrupted after a maximum of 3000ms. The next trial began after an inter trial interval (ITI) of 1000 ms starting with the execution of the second response. Error feedback was presented centrally for 500 ms during the ITI after omitted responses as well as after incorrect stimulus discrimination and consisted of the German words 'ZU LANGSAM' (too slow) or 'FALSCH' (incorrect), respectively.

Please insert Figure 1 here

Each participant performed a single experimental session for approximately 60 to 75 minutes. Sessions started with a practice phase consisting of nine single task blocks with 16 trials each and with three blocks for each the tone, the digit, and the letter task. The high number of practice blocks for each single task was applied in order to guarantee that participants represented both visual tasks as distinct tasks with two S-R mappings each, rather than integrating them into one visual task with four S-R mappings (Dreisbach, Goschke, & Haider, 2006, 2007). The trial timing of single-task trials was similar to DT trials, with the exception that only one stimulus was presented and that the response period started after the offset of the first stimulus. After single task blocks, participants were familiarized with the DT procedure in three practice blocks consisting of 32 trials. In the subsequent main part of the experiment, participants performed twelve DT blocks with 65 trials each.

Importantly, DT trials could differ in two succeeding trials with respect to two independent characteristics, i.e. the presentation order of the auditory and visual stimuli as well as the specific visual component task. In 50 % of trials, the auditory stimulus while in the other 50 % of trials a visual target stimulus was presented first. As a result, in the second of two succeeding trials, the order of stimulus presentation was either repeated (same-order trials) or reversed (different-order trials) relative to the preceding trial. The probability of both, same-order and different-order trials was .5. In addition, in 50 % of the trials, the digit task was presented as the visual component task while in the other 50 % the letter task was presented. As a result, the specific visual component task repeated in half of the trials, while it was changed in the other half of trials. This resulted in a 2 (task-repetition trials vs. task-change trials) × 2 (same-order vs. different-order trial) design. Probabilities for all factor combinations were balanced throughout the entire experiment.

Results

For all analyses, practice trials and the first trial of each experimental block were excluded. Additionally, for RT analyses, trials with erroneous (discrimination errors and task-order reversals) or omitted responses (*mean*[*m*] = 18 %) and trials with RTs longer or shorter than +/- 2.5 standard deviations for each participant and condition (*m* = 2.1 %) were discarded. RTs, error rates, and task-order reversal rates were aggregated across trials with the auditory or the visual task as task 1 as well as across trials including the digit or the letter task, respectively. RTs and error rates, separately for task 1 (RT1) and task 2 (RT2), and task-order reversal rates were analyzed using an ANOVA with the within-subjects factors TASK (task-repetition trials, task-change trials) and ORDER (same-order trials, different-order trials).

In order to provide additional evidence for either the separate or integrated task-order set hypothesis, we also applied a Bayesian repeated-measures ANOVA using JASP software (van den Bergh et al., 2020). In particular, this was done in order to provide an even stronger test for the interaction between the factors TASK and ORDER. A significant TASK × ORDER interaction would support the integrated task-order set hypothesis, which predicts performance benefits for same-order compared with different-order trials only the under the condition of a repeated visual component task but not under the condition of a changed visual component task. Note, however, that the separated task order set hypothesis does not predict a null hypothesis for the TASK × ORDER interaction per se; rather, it predicts a performance benefit for same-order relative to different-order trials for the condition of a repeated as well as for the condition of a changed component task. For the purpose of the Bayes analysis, we calculated the posterior probabilities of a model including only the main effects of the factors TASK and ORDER and compared it to a model containing the main effects of these two factors as well as their interaction. Importantly, the Bayes factor BF_{01} provides information which of these two models better fits the data, with values smaller than 1 providing evidence for the model including the interaction effect and values larger than 1 providing evidence for a model not including this interaction (Raftery, 1995; Wagenmakers, 2007).

Task 1 Analyzing RT1, we found a significant effect of the factor TASK, $F(1, 18) = 25.63$, $p < .001$, $\eta_p^2 = .59$. As can be seen in Figure 2, in trials in which the visual task has changed relative to the previous trial, RT1 was slower (m = 1070 ms) compared to trials with repeated component tasks (m = 1037 ms, see Figure 2) replicating previous studies (e.g. Hirsch, Nolden, & Koch, 2017). In addition, the factor ORDER also reached significance, $F(1, 18) = 30.98$, $p < .001$, $\eta_p^2 = .63$, indicating a performance benefit for same-order (m = 1026 ms) versus different-order trials (m = 1082 ms) and confirming the operation of the task-order set (Kübler et al., 2018; Luria & Meiran, 2003).

Crucially, this performance benefit for same-order trials in RT1 was found irrespective of whether or not the particular visual component task had been repeated (m = 63 ms, $t(18) = 4.60$, $p < .001$) or changed (m = 49 ms, $t(18) = 5.32$, $p < .001$) compared to the previous trial. This was also confirmed by the non-significant interaction of the factors TASK and ORDER, $F(1, 18) = 1.46$, $p = .24$, $\eta_p^2 = .08$, suggesting similar performance benefits for same-order trials under both conditions. This was also supported by a Bayes Factor of $BF_{01} = 2.14$ from the respective model comparison providing evidence for a model only containing the main effects TASK and ORDER without further specifying an interaction of these two factors. Overall, these results are in line with the separated order set hypothesis suggesting that the order set only contains information about the processing order of tasks.

Regarding the error rate in task 1, we only found a significant effect of the factor TASK, $F(1, 18) = 6.64$, $p < .05$, $\eta_p^2 = .27$; error rates increased in trials with a changed (m = 3.0 %) relative to trials with a repeated task visual component task (m = 2.4 %). The factor ORDER, $F(1, 18) = 1.76$, $p = .20$, $\eta_p^2 = .09$, and the interaction TASK × ORDER, $F(1, 18) < 1.00$, $p = .97$, $\eta_p^2 < .01$, were not significant. The non-significant interaction is also supported by a Bayes factor of $BF_{01} = 3.80$ providing positive evidence for a model that does not specify the interaction of the two factors TASK and ORDER. Error rates can be found in Table 1.

Task 2 For RT 2 we found similar results as for RT1. Changing the visual component task relative to the preceding trial resulted in slowed RT2 (1157 ms) compared to repeating the visual task (*m* = 1117 ms), $F(1, 18) = 43.37$, $p < .001$, $\eta_p^2 = .71$. Furthermore, RT2 was faster in same-order trials (*m* = 1107 ms) compared to different-order trials (1167 ms), $F(1, 18) = 37.21$, $p < .001$, $\eta_p^2 = .67$, again indicating the operation of the task-order set.

Importantly, also for RT 2, the performance benefit for same-order trials was evident in the condition of a repeated (*m* = 67 ms, $t(18) = 4.69$, $p < .001$) and the condition of a changed visual component task (*m* = 52 ms, $t(18) = 5.58$, $p < .001$). As was indicated by the non-significant interaction TASK × ORDER, $F(1, 18) = 1.07$, $p = .31$, $\eta_p^2 = .06$, this benefit for same-order relative to different-order trials did not differ between both conditions. This was also supported by a model comparison that provided a Bayes factor of $BF_{01} = 2.20$ favoring a model specifying only the main effects of TASK and ORDER without including the interaction TASK × ORDER.

Analyzing accuracy in task 2, we found significant effects of the factors TASK, $F(1, 18) = 12.25$, $p < .01$, $\eta_p^2 = .41$, and ORDER, $F(1, 18) = 6.40$, $p = .02$, $\eta_p^2 = .26$. Error rates increased from trials with a repeated (*m* = 3.5 %) to trials with a changed visual component task (*m* = 4.8 %) and from same-order (*m* = 3.6 %) to different-order trials (*m* = 4.6 %). The interaction of these two factors was not significant, $F(1, 18) = 1.44$, $p = .29$, $\eta_p^2 = .06$. Also, the respective Bayesian model comparison provided evidence for a model that does not include this interaction $BF_{01} = 2.09$. In sum, similar to task 1, task 2 data are in favor of the separated task-order set hypothesis suggesting that the task-order set only specifies the processing order of the two tasks.

Please insert Figure 2 here

Task-order reversals Task-order reversal rates can be found in Table 1. We did not find a significant effect of the factor TASK, $F(1, 18) < 1.00$, $p = .78$, $\eta_p^2 < .01$. As indicated by the significant effect of the factor ORDER, $F(1, 18) = 33.73$, $p < .001$, $\eta_p^2 = .65$, task-order reversal rates were lower in same-order trials (m = 9.7 %) compared with different-order trials (m = 13.2 %) replicating earlier studies (e.g. Kübler et al., 2019). Importantly, the benefit in task-order reversals for same-order relative to different-order trials was evident in trials with a repeated (m = 2.9 %, $t(18) = 4.37$, $p < .001$) and in trials with a changed visual component task (m = 4.1 %, $t(18) = 4.20$, $p = .001$) relative to the preceding trial. Furthermore, this benefit did not differ between both conditions as indicated by the non-significant TASK × ORDER interaction, $F(1, 18) = 1.05$, $p = .32$, $\eta_p^2 = .06$. This was also supported by a Bayes Factor of $BF_{01} = 2.24$ from the respective model comparison providing evidence for a model that does not include an interaction of these two factors. Thus, task-order reversal rates were lower in same-order compared with different-order trials – even if the visual component task had changed. Similar to RT data, the results of the task-order reversal rates also support the assumption of the separated task-order set hypothesis.

Please insert Table 1 here

Discussion

The aim of Experiment 1 was to identify the organization of order and specific task information in DT situations by varying task order *and* implementing a changing visual component task. As a result, RT data of task 1 and task 2 as well as task-order reversal rates indicate a performance benefit for same-order trials compared to different-order trials (Kübler et al., 2018; Luria & Meiran, 2003, 2006). Importantly, this performance benefit was the same in trials with a repeated *and* in trials with a changed visual component task relative to the preceding trial as was also indicated by the respective Bayesian model comparisons. This data pattern is in line with the separated task-order set hypothesis according to which the order set only contains order information but not task-specific information. If the task-order set contained order information as well as specific task information, as it is proposed by the integrated task-order set hypothesis, we should have observed a performance benefit for same-order trials only in the case of a repeated but not in the case of changed visual component task. This is so because in this case the task-order set of the previous trials would not specify the correct component task and, thus, could not be re-applied – even if task order was repeated. As we found a performance benefit for same-order trials even under the condition of a changed visual component task we can, thus, exclude the assumption that the task-order set specifies the component tasks and reject the integrated order set hypothesis.

Importantly, in Experiment 1, we used a random-order DT consisting of one auditory tone discrimination task and two randomly changing visual component tasks, a letter and a digit discrimination task. Thus, the applied DT from Experiment 1 constitutes a very specific and narrowly circumscribed DT situation. However, we cannot exclude that the results of the first experiment supporting the separated order set hypothesis might only be valid for the specific case of this applied DT with two changing visual and one constant auditory component task. In fact, this specific task composition might have favored the acceptance of the separated over the integrated task-order set hypothesis for several reasons and, thus, it is not clear whether the corresponding results can be generalized to other DT situations.

Ad one, the changing component task applied in Experiment 1 was a visual-manual task and, therefore, it needs to be assessed whether a similar data pattern can be observed, if the auditory-manual component task will change while the visual-manual task remains constant. The processing demands of visual-manual component tasks may not be comparable to those of auditory-manual component tasks as the latter require a transmission from auditory information to the manual motor response systems (Hazeltine, Ruthruff, & Remington, 2006; Huestegge & Koch, 2010; Stelzel, Schumacher, Schubert, & D'Esposito, 2006). This might prevent the separate representation of component task information and task-order information in the case of a changing auditory-manual but a constant visual-manual component task. Ad two, in DTs consisting of both visual and auditory component tasks, processing of the auditory task can be prioritized compared to the visual task (Maquestiaux, Didierjean, Ruthruff, Chauvel, & Hartley, 2008; Maquestiaux, Laguë-Beauvais, Bherer, & Ruthruff, 2008), potentially due to the alerting characteristics of auditory stimuli. This might also prevent the separate representation of auditory task information and order information.

To tackle these issues and in order to investigate, if the assumptions of the separated task-order set hypothesis can also be transferred to a DT situation with a different task composition, we conducted Experiment 2. In this experiment we applied a random-order DT with one visual and two changing auditory tasks – a pitch and a timbre discrimination task.

Experiment 2

In Experiment 1, we found evidence for the separated task-order set hypothesis assuming that the order set only contains order information. The aim of Experiment 2 was to test, whether we can generalize this pattern to DT situations with different task compositions. This was necessary as the specific task compositions in Experiment 1 might have favored the acceptance of the separated over the integrated task-order set hypothesis. For this purpose, in Experiment 2, participants performed a random-order DT consisting of one visual and one of two auditory component tasks - either a pitch or a timbre discrimination task. Based on the separated task-order set hypothesis, we expected a performance benefit for same-order versus different-order trials in trials with a repeated and a changed auditory component task. In contrast, according to the integrated task-order set hypothesis, there should be a performance benefit for same-order trials only when the auditory component task has been repeated but not when it has been changed relative to the preceding trial.

Materials and Method

Participants In Experiment 2, we tested twenty-four participants from the Martin-Luther Universität Halle-Wittenberg. Analogously to Experiment 1, the required sample size was estimated for a reliable effect of the factor ORDER using the G*Power software (Faul et al., 2007). For this power analysis of Experiment 2, the specification of parameters was similar to the one of Experiment 1 with the difference that the estimated effect size was specified as of $\eta_p{}^2 = .24$ (which corresponds to an effect size of $f = .56$). This effect size was estimated based on analyzing data from pre-experimental piloting with four participants (which were not included in the final sample of twenty-four participants). This a-prior power analysis yielded a required sample size of N = 23. We increased this required sample size to 24 in order to compensate for potential dropout due to low accuracy or similar. Please also note that pilot participants produced more discrimination errors (m = 8.5 %) and task-order reversals (m = 19.6 %) than participants of Experiments 1 (discrimination errors: m = 5.4 %; task-order reversals: m = 17.6 %). This suggests than, when introducing two different auditory component tasks rather than two different visual component tasks, participants seem to have slightly more difficulties with performing the DT. Considering the a-priori power analysis as well as the increased difficulty, we believe that the increase in sample size from Experiment 1 to Experiment 2 is necessary to guarantee sufficient power for testing the effects of interest.

Participants (17 female) mean age amounted to 25.5 years (*SD* = 6.0 years). All participants had normal or corrected-to-normal vision, were right handed and received 8 euros per hour or course credit for their participation. Approval of the local ethics committee was obtained and written informed consent was collected from each participant. Data of two participants were excluded since they did not meet the inclusion criterion of 60 % of correct trials (only 50 % and 53 % of correct trials).

Apparatus and tasks The apparatus was the same as in Experiment 1. In contrast to Experiment 1, participants performed a DT consisting of one visual task and one of two auditory tasks. For the visual task, we used the digit stimuli and the corresponding response keys from Experiment 1. For the auditory tasks, we either presented a *pitch task*, in which participants were required to respond to a tone with low or high pitch (see Experiment 1), or a *timbre task*, in which participant were asked to discriminate a piano or a trombone sound. Responses for the *pitch* and the *timbre task* were mapped on the same response keys: Participants were requested to press the ‘Y’ key in response to the low pitch and trombone sound, and the ‘X’ key in response to the high pitch and piano sound with their right index and middle fingers, respectively.

Design and procedure The design and procedure was analogous to Experiment 1.

Results

Data pre-processing and analyses were analogue to Experiment 1. For RT analyses, trials with erroneous (discrimination errors and task-order reversals) or omitted responses (m = 28.0 %) as well as trials with RTs longer or shorter than +/- 2.5 standard deviations for each participant and condition (m = 2.2 %) were discarded from analyses.

Task 1 Analyses of RT1 revealed similar findings compared with Experiment 1. The effect of the factor TASK was significant, $F(1, 21) = 30.83$, $p < .001$, $\eta_p^2 = .60$. RT1 was slower in trials with a changed (m = 1117 ms) relative to trials with a repeated auditory component task (m = 1057 ms) replicating earlier findings (Hirsch, Nolden, & Koch, 2017; Hirsch, Nolden, Philipp, et al., 2017). Furthermore, we found a significant main effect of

TASK ORDER, $F(1, 21) = 31.27$, $p < .001$, $\eta_p{}^2 = .60$: RT1 was faster in same-order trials (m = 1057 ms) compared with different-order trials (m = 1118 ms).

Importantly, the performance benefit for same-order trials versus different-order trials in RT1 occurred in trials with a repeated (m = 69 ms, $t(21) = 6.14$, $p < .001$) and a changed (m = 52 ms, $t(21) = 3.90$, $p < .01$) auditory component task relative to the preceding trial. This was also confirmed by the non-significant interaction of the factors TASK and ORDER, $F(1, 21) = 2.28$, $p = .15$, $\eta_p{}^2 = .01$,) and, again, favors the separated over the integrated task-order set hypothesis. The lack of a significant effect for this interaction was further supported by the respective Bayesian model comparison with $BF_{01} = 2.41$ providing evidence for model containing only the main effects of TASK and ORDER without specifying the interactions of these two factors. All RTs for Experiment 2 can be found in Figure 3.

For accuracy in task 1 (see Table 1), the effect of TASK, $F(1, 21) = 2.57$, $p = .12$, $\eta_p{}^2 = .11$, did not reach significance levels. We observed a significant effect of the factor ORDER, $F(1, 21) = 36.75$, $p < .001$, $\eta_p{}^2 = .64$, indicating an increase in error rates for task 1 from same-order (m = 3.6 %) to different-order trials (m = 5.0 %). This increase in error rates did not differ between trials with a repeated and changed auditory component task, since the interaction effect TASK × ORDER, $F(1, 21) < 1.00$, $p = .50$, $\eta_p{}^2 = .02$, was not significant. In line with this interpretation, with Bayes factor of $BF_{01} = 2.62$, the respective model comparison provided further evidence in favor of a model that does not include this interaction.

Task 2 Analyses of RT2 replicated the results of RT1. Again, we found a significant increase in RT2 when the auditory component task had changed (m = 1233 ms) relative to the previous trial compared to when the auditory task had been repeated (m = 1163 ms), $F(1, 21) = 33.12$, $p < .001$, $\eta_p{}^2 = .61$. Furthermore, the significant effect of ORDER, $F(1, 21) = 24.27$,

$p < .001$, $\eta_p^2 = .54$, indicated a performance benefit for same-order ($m = 1168$ ms) relative to different-order trials ($m = 1228$ ms).

Crucially, we observed this performance benefit in trials with a repeated ($m = 69$ ms, $t(21) = 5.84$, $p < .001$) and in trials with a changed auditory component task ($m = 49$ ms, $t(21) = 3.12$, $p < .01$) relative to the preceding trial. Furthermore, these performance benefits for same-order trials were similar for the two condition as supported by the non-significant interaction TASK × ORDER, $F(1, 21) = 1.76$, $p = .20$, $\eta_p^2 = .08$. This interpretation is also confirmed by a Bayes factor of $BF_{01} = 2.52$ favoring a model containing only the main effects of these two factors compared to a model containing these main effects as well as their interaction.

Analyzing error rates in task 2, we did not find any effects on task accuracy, neither for the factor TASK, $F(1, 21) < 1.00$, $p = .88$, $\eta_p^2 < .01$, ORDER, $F(1, 21) = 1.75$, $p = .20$, $\eta_p^2 = .08$, nor for their interaction, $F(1, 21) < 1.00$, $p = .60$, $\eta_p^2 = .01$. Also, with a Bayes factor of $BF_{01} = 2.82$, the respective model comparison provided evidence for a model that does not include this interaction. In sum, RT benefits for same-order relative to different-order trials in task 1 task 2 were evident (and similar) under the condition of a repeated and a changed auditory component task, favoring the separated over the integrated task-order set hypothesis.

Please insert Figure 3 here

Task-order reversals The significant effect of the factor TASK, $F(1, 21) = 77.81$, $p < .001$, $\eta_p^2 = .79$, indicated that task-order reversal rates were increased in trials with a changed ($m = 19.4$ %) relative to trials with a repeated ($m = 16.7$ %) auditory component task. In addition, there was a trend for an effect of the factor ORDER on task-order reversal rates, *F*(1,

21) = 3.51, p = .07., $\eta_p{}^2$ = .14. This nearly significant main effect was further specified by the significant interaction TASK × ORDER, $F(1, 21) = 215.56$, $p < .001$., $\eta_p{}^2$ = .91. Also, the Bayes analyses provided strong evidence for this interaction with $BF_{01} < .001$.

Further analyses revealed that, under the condition of a changed auditory component task, task-order reversals increased from same-order (m = 17.1 %) to different-order trials (m = 21.8 %), $t(21) = 12.51$, $p < .001$. Importantly, this finding contradicts the assumption of the integrated task-order set hypothesis, which predicts no difference in task-order reversal rates between same-order and different-order trials when the specific auditory task has changed relative to the preceding trial. Thus, also based on the task-order reversal rates, we can reject the integrated task-order set hypothesis. In addition, in trials with a repeated auditory component task, task-order reversal rates decreased from same-order trials (m = 19.7 %) to different-order trials (m = 13.7 %), $t(21) = 10.20$, $p < .001$.

Discussion

In Experiment 2, we observed a performance benefit for same-order compared to different-order trials in task 1 and task 2 - even when the auditory component task had changed relative to the preceding trial as was also confirmed by Bayes analyses. Similarly, we also found reduced task-order reversal rates in same-order compared with different-order trials in trials with a changed auditory component task. Thus, also based on data of Experiment 2 we can reject the integrated task-order set hypothesis, which would have predicted no performance benefits for same-order trials when the specific auditory component task has changed relative to the preceding trial. Instead, the current results rather support the separated task-order set hypothesis, according to which task-order and specific task information is represented separately. Importantly, in Experiment 2, we confirmed the predictions of the separated order set hypothesis in a DT situation with two changing auditory tasks. Thus, the results for Experiment 1 are not specific for the applied DT situations with two changing visual component tasks. Instead, we demonstrated that the separated task-order set hypothesis also holds true in a broader range of DT situations with different task compositions.

However, in the first two experiments, we only varied one of the component tasks randomly from trial to trial, either the auditory or the visual component task. Consequently, demands on maintaining component task information in working memory (WM) were relatively low because only three different tasks had to be held active during DT processing (Schubert & Strobach, 2018). Crucially, this relatively low demand on maintaining component task information in WM might have advantaged the acceptance of the separated over the integrated task-order set hypothesis. In particular, because WM is characterized by limited capacity (Baddeley, 2003; Cowan, 2010), the amount of task information that can be maintained active in WM during task processing is restricted (Brass, Liefooghe, Braem, & De Houwer, 2017; Oberauer, 2009). Applying three component tasks in Experiment 1 and Experiment 2, however, might have provided a situation that did not exceed available WM resources and, thus, allows for the separate representation of specific task information and task-order information. Increasing the number of component tasks, in contrast, might push WM to its limits. As a result, WM capacity might not be sufficient to maintain specific task as well as order information separately. In this case, increasing WM load may force participants to combine specific component task information und task-order information into an integrated task-order set in order to maintain all information necessary to deal with the DT at hand. Theoretically, in this situation, combining component task and task-order information in one integrated order set might be beneficial for a limited capacity system because less task components, i.e. only one integrated order set rather than separate task sets and task-order sets, have to be maintained concurrently in WM. Importantly, the integration of component task and task-order information into one representation is the core assumption of the integrated task-order set hypothesis. Thus, the results of Experiment 1 and Experiment 2 providing evidence for the separated task-order set hypothesis might be restricted for the special case of a DT with relatively low demands on task set maintenance. Therefore, in Experiment 3, we tested whether increasing these demands compared with previous experiments would affect the organization of task information in DTs. To do so, we applied a random-order DT with two randomly changing component tasks (in total four component tasks). Thus, not only one component task but both the visual *and* the auditory task could either repeat or change in two succeeding trials.

Experiment 3

In the previous experiments, we varied only one of the component tasks, either the visual or the auditory component task, and found evidence for the separated task-order set hypothesis. Importantly, by applying this approach, participants had to maintain only three component tasks active posing relatively low demands on WM (Schubert & Strobach, 2018). Such relatively low WM demands in Experiments 1 and 2 might have enabled participants to store and process task-order information separately to the specific task information. In this case, the results of the previous experiments, could only be regarded as exceptions to DTs with low amount of WM load. A generalization to a larger range of DT situations, which are not restricted by these low demands, would not be possible.

Thus, in Experiment 3, we tested whether the findings of the previous two experiments can also be generalized for DT situations with higher demands to maintain specific task information active in WM; we did so by including four different component tasks, two visual and two auditory tasks. The resulting increased amount of WM load might disenable participants to represent component task information and task-order information in a separated fashion. Instead, increasing the amount of WM load could force the participants to integrate both, component task information and task-order information into one joint representation in order to avoid an overload of available WM resources. This, however, would be consistent with the integrated task-order set hypothesis. Importantly, if this was the case, we should find no performance benefits for same-order compared to different-order trials when one or both component tasks changed relative to the preceding trial. If, on the contrary, a separate representation of task-order and task specific information also occurs under the condition of increased WM load due to the increased amount of component task information, then we should find performance benefits for same-order relative to different-order trials irrespective of repeated or changed component tasks in the current experimental situation.

The current design of Experiment 3 allows us for testing an additional research question concerning how participants change the representations for the two component tasks within a DT situation. In particular, we aim to test whether the change of component task representation occurs separately for each component task, or whether task information is changed conjointly for both tasks. If component task representations are changed separately, this would lead to an increase in RTs from trials with one compared to trials with two task changes relative to the preceding trials. This is so because in the former situation only one component task representation needs to be changed, whereas in the latter situation two component task representations need to be changed resulting in increased processing times. Alternatively, one might assume that the two component tasks are represented as a conjoint pair of two component tasks, which CANNOT be changed partially but only as a whole. In this case, we should expect that participants need to change the representation specifying the entire task composition consisting of two component tasks when only one task changes as well as when two tasks change relative to the preceding trial. This would predict no response time increase for trials with two compared to trials with one task change.

Materials and Method

Participants As the demands of the DT situation in Experiment 3 were additionally increased due to the application of four instead of three component tasks compared to previous experiments, we increased the power by further increasing the number of participants. In addition, the decision about the increase in participant number was further supported by an additional a-priori power analysis for a reliable effect of the factor ORDER using the G*Power software (Faul et al., 2007). For this power analysis of Experiment 3, the specification of parameters was similar to the previous experiments, with the difference that the estimated effect size was specified as of $\eta_p{}^2 = .21$ (which corresponds to an effect size of $f = .51$). This effect size was estimated based on analyzing data from pre-experimental piloting with four participants (which were not included in the final sample of twenty-eight participants). This a-prior power analysis yielded a required sample size of N = 27. We increased this required sample size to 28 in order to compensate for potential dropout due to low accuracy or similar. Participants (24 female; mean age of $m = 21.4$ years [$SD = 3.2$ years]) were recruited at the Humboldt-Universität zu Berlin. Similar to Experiments 1 and 2, all participants had normal or corrected-to-normal vision, were right handed and received 8 euros per hour or course credit for their participation. Approval of the local ethics committee was obtained and written informed consent was collected from each participant. Data of 3 participants were excluded as they did not meet the inclusion criterion of 60 % of correct trials (only 40 %, 48 % and 57 % of correct trials, respectively).

Apparatus and tasks The apparatus was the same as in the previous experiments. However, in order to create a DT consisting of four potential component tasks, we combined the *digit* and the *letter* task as well as the *timbre* and *pitch* task from Experiment 1 and 2.

Design and procedure The trial structure was the same compared to Experiment 1 and Experiment 2. At the beginning of each session, participants performed three single task blocks à 16 trials for each component task which were followed by three DT blocks consisting of 32 trials each. In the main part of the experiment, participants were presented 15 DT blocks with 65 trials each. In total, the experiment lasted for approximately 90 minutes. As in

Experiment 3 both the auditory and the visual component task varied randomly from trial to trial, the factor TASK was manipulated on three levels: Both tasks could be repeated (i.e. no task changes), only one of the component tasks, either the auditory or the visual task, could change, or both tasks could change relative to the preceding trial. This resulted in a 3 (task-repetition trials, trials with one task change, trials with two task changes) × 2 (same-order vs. different-order) design. Probabilities for each factor combination were counterbalanced.

Results

Data analyses and pre-processing were performed in analogy to Experiment 1 and Experiment 2 with the following specifications. For RT analyses, trials with erroneous (discrimination error or task-order reversal) or omitted responses (18,6 %) as well as trials with RTs longer or shorter than +/- 2.5 standard deviations for each participant and condition (1.9 %) were discarded from analyses. RTs and error rates, separately for task 1 (RT1) and task 2 (RT2), as well as task-order reversal rates were analyzed using a 3 × 2 ANOVA with the within-subjects factors TASK (task-repetition trials, trials with one task change, trials with two task changes) and ORDER (same-order trials, different-order trials).

Task 1 Analyses on RT1 showed a significant effect of the factor TASK, $F(2, 48) = 30.72$, $p < .001$, $\eta_p^2 = .56$. RT1 slowed from trials with two task repetitions ($m = 989$ ms) to trials with one task change relative to the preceding trial ($m = 1036$ ms, $t(24) = 5.46$, $p < .001$). RTs were further slowed from trials with one task change to trials with two task changes ($m = 1077$ ms, $t(24) = 4.71$, $p < .001$) suggesting that changing two component tasks results in longer processing times than changing only one task. Importantly, this suggests that component task representation can be changed separately and individually for each task.

Also, the factor ORDER affected RT1, $F(1, 24) = 58.89$, $p < .001$, $\eta_p^2 = .71$: RT1 was faster in same-order trials ($m = 992$ ms) compared with different-order trials ($m = 1076$ ms). Further analyses demonstrated that these performance benefits for same-order trials occurred irrespective of repeated or changed component tasks. In trials with two repeated tasks relative to the preceding trial, RT1 increased from same-order trials (*m* = 938 ms) to different-order trials (*m* = 1041 ms), $t(24) = 7.36$, $p < .001$ (see Figure 4). Similarly, when one task changed relative to the preceding trial, RT1 increased from same-order (*m* = 984 ms) to different-order trials (*m* = 1087), $t(24) = 7.39$, $p < .001$. Also, when both tasks changed, there was a performance benefit for same-order (*m* = 1055 ms) relative to different-order trials (*m* = 1099 ms), $t(24) = 3.10$, $p < .01$. In other words, the performance benefit for same-order trials was observable irrespective of whether no, only one, or both tasks have changed again supporting the assumptions of the separated task-order set hypothesis.

However, although significant in all three conditions, the performance benefit for same- compared to different-order trials was significantly reduced in trials with two task changes (*m* = 44 ms) relative to trials with one changed task (*m* = 103 ms, $t(24) = 3.50$, $p < .01$) and trials with two repeated tasks (103 ms, $t(24) = 3.49$, $p < .01$); the latter two conditions did not differ, $t(24) < 1.00$, $p > .99$. This result pattern was also confirmed by interaction of the factors TASK and ORDER, $F(2, 48) = 9.68$, $p < .001$, $\eta_p^2 = .29$. Similarly, Bayes analyses provided moderate evidence for a model which includes this interaction compared to model which does not, $BF_{01} = .17$, This reduced, albeit still significant, performance benefit for same-order trials in trials with two task changes relative to trials with only one or no task change might be attributed to the increased processing demands due to replacing the task information of two tasks (see Discussion).

Error rates in task 1 were not affected by the factor TASK, $F(2, 48) = 1.53$, $p = .23$, $\eta_p^2 = .06$. Error rates slightly increased from same-order trials (*m* = 3.9 %) to different-order trials (*m* = 4.6 %), $F(1, 24) = 4.73$, $p = .04$, $\eta_p^2 = .17$ (see Table 2). The interaction TASK × ORDER just failed to reach significance, $F(2, 48) = 3.22$, $p = .06$, $\eta_p^2 = .12$. The latter was also confirmed in the respective model comparison with a Bayes factor of $BF_{01} = 1.35$.

Task 2 Analyses of performance in task 2 revealed a similar pattern as for task 1. RTs slowed down with increasing number of task changes, $F(2, 48) = 34.29$, $p < .001$, $\eta_p{}^2 = .59$: RT2 increased from trials with two task repetitions (*m* = 1000 ms) to trials with only one task change (*m* = 1055 ms, $t(24) = 6.32$, $p < .001$). RTs further increased from trials with only one task change to trials with two task changes (*m* = 1105 ms, $t(24) = 4.65$, $p < .001$) again suggesting that that component task representation can be changed separately and individually for each task.

In addition, we observed a RT benefit for same-order trials (*m* = 1010 ms) in comparison to different-order trials (*m* = 1097 ms). This was was confirmed by the significant effect of the factor ORDER, $F(1, 24) = 54.10$, $p < .001$, $\eta_p{}^2 = .69$. As can be seen in Figure 4, this performance benefit for same- versus different-order trials was evident in trials with two task repetitions (same-order trials: *m* = 943 ms, different-order trials: *m* = 1058 ms, $t(24) = 7.60$, $p < .001$), in trials with only one task change (same-order trials: *m* = 1003 ms, different-order trials: *m* = 1108 ms, $t(24) = 7.90$, $p < .001$), and in trials with two task changes (same-order trials: *m* = 1084 ms, different-order trials: *m* = 1125 ms, $t(24) = 2.74$, $p = .01$), again providing evidence for the separated order set hypothesis.

Similarly to RT 1, the benefit in RT2 for same-order relative to different-order trials was modulated by the number of task changes, $F(2, 48) = 16.29$, $p < .001$, $\eta_p{}^2 = .404$. This significant interaction was also confirmed by a Bayes factor of $BF_{01} = .06$ when comparing the models with and without specifying this interaction. Further analyses revealed that the benefit for same-order trials was significantly reduced in trials with two changed component tasks (40 ms) relative to trials with repeated tasks (*m* = 115 ms), $t(24) = 4.73$, $p < .001$, and trials with only one changed task (*m* = 105 ms), $t(24) = 4.08$, $p < .001$); the performance benefit for same-order trials did not differ between the latter two conditions, $t(24) = 1.04$, $p = .31$).

Analyzing error rates in task 2, we only found an effect of the factor TASK, $F(2, 48) = 7.15$, $p < .01$, $\eta_p^2 = .23$. As can be seen in Table 2, error rates in trials with two repeated tasks (*m* = 5.1 %) were lower compared to trials with one (*m* = 5.8 %, $t(24) = 2.18$, $p < .05$), and trials with two changed tasks (*m* = 6.6 %, $t(24) = 3.52$, $p < .01$); the comparison of the latter two conditions also showed a descriptive difference but just failed to reach significance ($t(24) = 1.86$, $p = .08$). The factor ORDER, $F(1, 24) < 1.00$, $p = .64$, $\eta_p^{2\,2} = .01$, as well as its interaction with the factor TASK, $F(2, 48) = 2.18$, $p = .13$, $\eta_p^2 = .08$, $BF_{01} = 1.20$ were not significant.

Please insert Figure 4 here

Task-order reversals We found a main effect of TASK on task-order reversals, $F(2, 48) = 11.56$, $p < .001$, $\eta_p^2 = .33$: Task-order reversal rates slightly decreased from trials with repeated tasks (*m* = 10.3 %) to trials with one task change (*m* = 9.3 %, $t(24) = 2.82$, $p = .01$), and from trials with one task change to trials with two task changes (*m* = 8.0 %, $t(24) = 2.40$, $p < .05$). Additionally, we found a significant main effect of the factor ORDER, $F(1, 24) = 16.54$, $p < .001$, $\eta_p^2 = .41$.We observed significant increases in order reversal rates from same-order trials to different-order trials for trials with two repeated tasks (same-order trials: *m* = 7.1 %, different-order trials: *m* = 13.6 %, $t(24) = 4.54$, $p < .001$), for trials with only one task change (same-order trials: *m* = 7.0 %, different-order trials: *m* = 11.5 %, $t(24) = 4.53$, $p < .001$), and for trials with two task changes (same-order trials: *m* = 6.7 % different-order trials: *m* = 9.2 %, $t(24) = 2.04$, $p = .05$). Thus, in line with the separated task-order set hypothesis, we found increased task-order reversal rates for different- versus same-order trials irrespective of repeated or changed component tasks.

The main effect of ORDER was additionally modulated by the factor TASK, $F(2, 48) = 8.82$, $p = .001$, $\eta_p{}^2 = .27$, indicating a pattern of results mirroring those of RTs. This was also supported by the respective model comparison favoring a model that includes this interaction compared with a model that does not, $BF_{01} = .63$ The difference in task-order reversal rates between same-order and different-order trials was reduced for trials with two task changes ($m = 2.5$ %) relative to trials with only one task change ($m = 4.6$ %, $t(24) = 2.34$, $p < .05$) and relative to trials with two repeated tasks ($m = 6.5$ %, $t(24) = 3.97$, $p < .001$.) The comparison of the latter two conditions just failed to reach significance, $t(24) = 1.95$, $p = .06$. In sum, similar to RT data, order reversal rates increased from same-order to different-order trials irrespective of a repetition or change of the component tasks, again providing evidence for the separated order set hypothesis.

Please insert Table 2 here

Discussion

In Experiment 3, the visual and the auditory component task as well as task order varied randomly from trial. Importantly, we replicated the results of Experiment 1 and Experiment 2: Irrespective of whether no or one component task changed, performance benefits occurred for same-order relative to different-order trials. In addition, we also found a performance benefit for same-order trials when both tasks changed relative to the preceding trial. Again, this provides evidence for the separated task-order set hypothesis suggesting that the task-order set only contains order but not task-specific information. Thus, we can conclude that the results of Experiment 1 and Experiment 2 are not restricted to DT situations with only one variable component task. Instead, the results of Experiment 3 suggest that these previous findings can be generalized to DT situations with more than three component tasks and, thus, increased demands on maintaining task information in WM.

However, despite the finding of significant performance benefits for same-order trials irrespective of the repeated or changed component tasks, we found that this benefit was smaller for trials with two component task changes in comparison to with trials with only one or no task change. One reason for this reduction might be the increased demands in the former condition (see below). While in this condition participants were required to change task information of two component tasks, in both other conditions participants could re-apply specific task information of at least one or even both component tasks. As a result of these increased demands due to two task changes, it might be that participants could not capitalize as much on the task-order set of the previous trial as they did when only one or none of the tasks changed. Hence, performance benefits for same-order versus different-order trials were decreased when two tasks changed relative to the preceding trial. Note that this observation is in line with the assumption that increasing demands during DTs impairs the storage and maintenance of task information (Ellenbogen & Meiran, 2008; Schubert & Strobach, 2018)[1].

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[1] Importantly, in a replication experiment testing 20 participants from the Martin-Luther University Halle-Wittenberg (17 female, mean age of 22.5 years, exclusion of one participant after early abortion of the experiment) we found the identical pattern of results for RTs. An ANOVA with the within-subjects factors TASK (task-repetition trials, trials with one task change, trials with two task changes) and ORDER (same-order trials, different-order trials) across task 1 and task 2, demonstrated a significant effect of the factor TASK, $F(2, 36) = 80.88$, $p < .001$, $\eta_p^2 = .82$: RTs increased in a stepwise fashion from trials with no task change (1152 ms) over trials with one task change (1210 ms) to trials with two task changes (1278 ms). Additionally, irrespective of task changes, RTs increased from same-order trials (1178 ms) to different-order trials (1249 ms), $F(1, 18) = 51.12$, $p < .001$, $\eta_p^2 = .74$. However,

In addition, irrespective of task order, we found increased RTs in trials with two task changes compared to trials with only one task change relative to the preceding trial. This indicates changing two tasks compared to only one task results in increased processing times. These increased processing times are suggestive for the fact that component task representation can be changed separately for each component task: When two tasks change, participants seem to change two task representations. When only one task changes, in contrast, participants seem to only change one task representation As will be discussed further in the general discussion, this result suggests that specific task information is stored separately for each component task and that the task sets of the component tasks can be changed individually to a variable task composition.

General Discussion

Summary of the Results

The aim of the current study was to investigate the content of the task-order set as well as the organization of order and task-specific information in DT situations. In order to test between two different hypotheses, i.e. the integrated and the separated task-order set hypothesis, we applied a DT with variable task order and changing component tasks. The present findings demonstrate that, consistent with the separated task-order set hypothesis, the task-order set only contains information about the sequence of task processing; specific task information, such as the S-R mappings, is represented separately. In Experiment 1, this was indicated by the occurrence of performance benefits for same-order in comparison to different-order trials irrespective of a repeated or changed visual component task relative to the preceding trial. In Experiment 2, we replicated these results in a DT situation with variable task order and a changing auditory component task. This suggests that the findings of

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the this increase was smaller in trials with two task changes (28 ms) compared to trials with one (93 ms) and no task change (93 ms), $F(2, 36) = 9.56$, $p < .001$, $\eta_p^2 = .35$.

Experiment 1 are not restricted to DTs with changing visual component tasks but instead can be transferred to other DT situations with different component task compositions. Also, in Experiment 3 (and in an additional replication), we provided evidence for the separated task-order set hypothesis. Importantly, in this experiment we increased the amount of WM load compared to the preceding experiments by applying four rather than three changing component tasks. Here we also found performance benefits for same-order trials in trials with one and trials with two changed component tasks relative to the preceding trial. Importantly, this finding indicates that the separate representation of task specific and task-order information occurs irrespective of WM load. In sum, based on the results of all three experiments, we can reject the integrated task-order set hypothesis according to which task-order and specific task-information is blended together into an integrated task-order set. Instead, the current results favor the separated task-order set hypothesis suggesting the separate representations of order and component task information.

Distinct representations for order and task-specific information

Importantly, the results of all experiments, i.e. performance differences between same-order and different-order trials despite changed component tasks, were predicted by the separated task-order set hypothesis. According to this hypothesis, different elements of task-relevant information, i.e. order and task-specific information, are represented separately by different representational entities, i.e. the task-order set and the task sets of the component tasks, respectively. As a result, both either a change in task order or a change of the component tasks contributed independently to performance decrements relative to trials with repetitions of the task sequence as well as the component tasks. In contrast, the integrated task-order set hypothesis presupposes that the order set contains order as well as specific task information and integrates both types of information into one single representation, i.e. a *super task-order representation*. However, based on the given results, this was clearly not the case, as such an organization would cause a change of the order set as soon as one of the task elements, i.e. either task order or one of the component tasks, is changed. This is so because after a change of the component task the order set of the previous trial does not match the DT composition of the current trial. As a result, despite a repeated task order, under the condition of a changed component task, no performance benefits for same-order trials should have occurred. Thus, based on the presented observation of performance benefits for same-order trials after a component task change, we can confirm the separated task-order set hypothesis while rejecting the alternative hypothesis of an integrated task-order set.

The present study provides direct evidence for the fact that order information and specific task information in DT situations is represented separately by different representations, namely by the task-order set and the task sets of the component tasks, respectively. From a theoretical point of view, the notion of separate representations for task order on the one hand, and the component tasks constituting the DT on the other has already been suggested in previous studies on task scheduling (Luria & Meiran, 2003; Sigman & Dehaene, 2006; Szameitat et al., 2006). For example, in their study, Luria and Meiran (2006) applied a DT with variable task order (but constant component tasks) and compared performance differences between same-order and different-order trials. Similar to the current study, they found performance benefits for same-order compared with different-order trials. Based on their findings, they assumed distinct representations for task-sequence and task specific information regulating behavior in DT situations: During the course of a DT trial, first a task-order control structure, i.e. the task-order set, has to be activated based on the order of stimulus presentation. The order set then guides appropriate task processing by sequentially activating the task sets of the component tasks. By introducing changing component tasks in addition to a changing task order, we further tested this assumption. Importantly, our results are completely in line with the assumptions of Luria and Meiran

(2003) and provide further evidence for the existence of distinct representations for task-order and component task information in DT situations.

The results indicating the separate representation of order and task specific information are also consistent with findings from neuroimaging studies which showed that information about the task sequence and about the task characteristics, such as stimuli and motor responses, is represented by different brain regions. For example, in their study, Stelzel et al. (2008) independently manipulated component task difficulty and demands on task-order coordination during a DT situation. They showed that increasing component task difficulty by varying the number of S-R mappings resulted in increased activation in posterior parts of the inferior frontal sulcus (close to the precentral gyrus) and the anterior insula. Increasing the demands on task-order coordination by contrasting blocks with constant and random task order, on the other hand, resulted in increased neural activation in more anterior parts of the inferior frontal sulcus and the middle frontal gyrus. Importantly, this dissociation implies that distinct brain regions are recruited for the representation of order and task-specific information. Importantly, the notion of distinct brain regions that represent different types of information, i.e. task-order and component task information, is in line with the core assumptions of the separated task-order set hypothesis.

DT representations as an agglomeration of separate components

Overall, the observation that order and specific task information is stored separately in different representations is suggestive for the assumption that DTs are represented by a loose agglomeration of independent informational components. Each of this component specifies a particular type of information, e.g. the task-order set specifying order information and the task sets specifying component task information. This perspective of a DT representation as a bundle of different informational components is further supported by the results of Experiment 3. In addition to the observation that task-order and specific task information is stored separately, in this experiment, we provided evidence that, in DT with variable task sequences, also specific task information is represented separately for each component task. This is suggested by an increase in RTs from trials with one task change to trials with two task changes relative to the preceding trial. This pattern of results suggests that specific task information can be changed separately for each component task. Changing only one component task compared to the preceding trial requires the change of only one task set. This is less demanding than changing two component tasks, which requires the change of two task sets. Consequently, we found faster responses in the former compared to the latter condition. Importantly, this suggests that specific task information is represented separately by distinct task sets for the particular component tasks.

This assumption of separate component task representation is not entirely in line with a recent proposal on DT processing according to which specific task information for both component tasks is integrated into one *task-pair* representation containing information about both tasks all at once (Hirsch, Nolden, & Koch, 2017; Hirsch, Nolden, Philipp, et al., 2017; see also Lien & Ruthruff, 2004). However, if in the current DT situation component task information was stored in a task-pair representation, we should have found no performance difference between trials with one and trials with two task changes. This is so because in both conditions the task-pair representation of the preceding trial could not be re-applied. Even the change of only one component task requires the change of the entire task-pair representation irrespective of the fact that the other component task is repeated compared to the previous trial. However, the increase in RTs from trials with one to trials with two task changes rather suggests that specific task information can be individually changed for each component task.

Overall, the results of the current study findings indicate that different types of information relevant for appropriate performance in DT situations are represented separately on different levels, i.e. in the case of this study by the task-order set on the one hand, and by both task sets of the component tasks on the other hand. These results contribute to the questions of how to define a task constituted by multiple task components in multitasking situation, such as DTs (Koch, Poljac, Müller, & Kiesel, 2018). The present findings indicate, that a task is represented as an agglomeration of separate, co-equal representational components, e.g. the task-order set and task sets of the component tasks. Each of these representation components contains information about a distinctive task element, e.g. the order of task processing, stimulus information and the S-R mapping of the two component tasks, etc. Furthermore, this collection of different representation components can be flexibly changed on the spot by separately replacing individual elements in order to comply with changing task demands. Importantly, such a form of task representation allows for an efficient and parsimonious adjustment of the cognitive system to variable environmental demands during multitasking.

The notion of a task representation as a collection of distinct representational components is also in line with evidence from other multitasking paradigm beyond DT situations, such as the task switching paradigm. In this paradigm, participants alternate between two different tasks on a trial to trial basis with only on task presented on each trial (Kiesel et al., 2010; Vandierendonck, Liefooghe, & Verbruggen, 2010) – rather than two tasks as in a DT situation. In a specific version of this paradigm, the task itself and, importantly, an additional task component varies randomly from trial to trial (see for example Kleinsorge & Heuer, 1999; Philipp & Koch, 2010). For instance, Hübner, Futterer, and Steinhauser (2001; see also Rangelov et al., 2013), applied a task switching paradigm consisting of a parity and a magnitude task. In addition, the authors varied the stimulus dimension by presenting the respective numerals as large digits (global level) made up of smaller digits (local level). Participants had to switch between the parity and the magnitude task as well as the global and local stimulus dimension. Importantly, the authors showed that performance was decreased when both components, i.e. the task *and* the stimulus dimension, compared to when only one component, i.e. either the task *or* the stimulus dimension, changed. In analogy to the findings of the present study, these results indicate that, in task switching, task goals and stimulus features are represented as separate task components and that each of these components can be replaced individually. Thus, the current findings indicate that the idea of a task representation as a collection of different representational elements can be generalized to other multitasking paradigms beyond task switching situations.

Similarly, the current findings are also supported by existing computational models of cognitive control in multitasking situations (Logan & Gordon, 2001; Meiran, Kessler, & Adi-Japha, 2008) that assume independent control parameters for different stages of task processing, such as stimulus identification, input selection and response selection, but also planning the sequence of multiple component tasks. As these models assume that different control parameters are independent, adjusting relevant parameter values should also occur separately. Importantly, the notion of the independent adjustment of different control parameters corresponds to the core assumptions of an agglomerated task representation. Furthermore, in the context of this study, this idea is completely in line with the separated task-order set hypothesis, namely that order and specific task information is represented separately and can be modified independently.

Interestingly, prior research suggests that a DT representation as an agglomeration of separate informational components is not immutable but instead could be modified by different contextual factors influencing the organization of task relevant information. For example, in their study, Dreisbach et al. (2007) applied a task switching paradigm consisting of two tasks with four S-R mapping each. Importantly, in a learning phase, one group practiced the relevant S-R mappings with knowledge about the underlying task sets (i.e. two tasks with four S-R mappings), while the other group practiced them without this knowledge (eight S-R mappings). Importantly, in the following experimental phase, only the group with knowledge about the underlying task sets showed significant switch costs, whereas the group without this knowledge did not. According to the authors, this result suggests that both groups organized task information in different ways. Only the former group integrated the four S-R mappings for each task in a particular task set which resulted in significant switch costs. The group that did not receive knowledge about the underlying task sets seemed to simply apply single S-R rules without integrating them into two distinct task sets. Interestingly, when participants from this group received information about the underlying task sets later during the experiment, switch costs also occurred for this group. This suggests that, if knowledge is available, the organization of task representation can be flexibly changed and task sets can be built on the spot. In analogy, also in the current DT situation with variable task order, contextual factors or interventions might affect the organization of order and task specific information. For instance, introducing changing component tasks and variable task order with different instructions during the practice phase may change the way of how participants structure relevant task information. Alternatively, it is conceivable extensive DT training may allow participant to integrate order and task specific information into one representation. Future research is necessary in order to investigate how these and similar factors may influence the organization of task relevant information in multitasking situations.

Conclusion

In sum, the results of the current study provide valuable insight about the basal characteristics of mental task representations in DT situations. We demonstrated that the - order set only contains sequence information about two temporally overlapping tasks without further specifying these tasks. Specific task information, on the other hand, is specified by a separate representation. Thus, the current findings indicate that a DT is represented by an agglomeration of more or less independent informational components that can be reused or changed flexibly in the face of variable task constraints resulting in independent effects of task-order changes as well as component task alternations.

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Table Caption

Table 1: Mean rates of errors for the first task (task 1) and the second task (task 2) as well as of task-order reversals in % (and standard deviation) as a function of trial type and repetition versus change of the visual (Experiment 1) and the auditory (Experiment 2) task, respectively.

Table 2: Mean rates of errors for the first task (task 1) and the second task (task 2) as well as of task-order reversals in % (and standard deviation) from Experiment 3 as a function of trial type and repetition versus change of one or two component tasks.

Table 1:

	Experiment 1					
	errors task 1		errors task 2		order- reversals	
	trial type		trial type		trial type	
	same-order trial	different-order trial	same-order trial	different-order trial	same-order trial	different-order trial
task repetition	2.3 % (2.2 %)	2.6 % (2.0 %)	3.2 % (2.6 %)	3.8 % (2.5 %)	10.1 % (8.3 %)	13.0 % (9.5 %)
task change	2.8 % (1.7 %)	3.1 % (2.2 %)	4.1 % (2.4 %)	5.5 % (4.4 %)	9.4 % (8.2 %)	13.4 % (10.2 %)
	Experiment 2					
	errors task 1		errors task 2		order- reversals	
	trial type		trial type		trial type	
	same-order trial	different-order trial	same-order trial	different-order trial	same-order trial	different-order
task repetition	3.4 % (2.5 %)	4.6 % (2.8 %)	5.3 % (3.3 %)	6.1 % (4.6 %)	19.7 % (5.2 %)	13.7 (3.8 %)
task change	3.8 % (2.5 %)	5.4 % (3.3 %)	5.6 % (4.4 %)	6.0 % (4.0 %)	17.1 % (4.2 %)	21.8 % (4.6 %)

Table 2:

	errors task 1		errors task 2		order- reversals	
	trial type		trial type		trial type	
	same-order trial	different-order trial	same-order trial	different-order trial	same-order trial	different-order trial
task repetition	4.1 % (2.1 %)	4.0 % (3.1 %)	4.7 % (3.2 %)	5.6 % (3.5 %)	7.1 % (6.6 %)	13.6 % (9.9 %)
one task change	3.6 % (2.6 %)	4.5 % (2.9 %)	5.9 % (3.1 %)	5.6 % (2.9 %)	7.0 % (6.6 %)	11.5 % (9.5 %)
two task changes	3.9 % (2.8 %)	5.2 % (3.5 %)	7.2 % (4.0 %)	6.1 % (4.6 %)	6.7 % (6.4 %)	9.2 % (8.7 %)

Figure Caption

Figure 1: The time course of a DT trial as it was applied in all three experiments. Following a fixation cross (1000 ms) both stimuli were presented for 200 ms each separated by an SOA of 200ms. The maximum time for both responses was set to 3000ms. After an ITI of 1000ms the next trial started. ITI = inter-trial interval, SOA = stimulus onset asynchrony.

Figure 2: Mean RTs for task 1 and task 2 as a function of trial type and repetition versus change of the visual component task in Experiment 1. Error bars reflect the standard error of the mean. Asterisks indicate significant performance benefits (faster RTs) for same-order versus different-order trials (* = $p \leq .01$, ** = $p \leq .001$). Left panel: RTs for task 1, right panel: RTs for task 2.

Figure 3: Mean RTs for task 1 and task 2 as a function of trial type and repetition versus change of the auditory component task in Experiment 2. Error bars reflect the standard error of the mean. Asterisks indicate significant performance benefits (faster RTs) for same-order versus different-order trials (* = $p \leq .01$, ** = $p \leq .001$). Left panel: RTs for task 1, right panel: RTs for task 2.

Figure 4: Mean RTs for task 1 and task 2 as a function of trial-type and repetition versus change of one or two component tasks in Experiment 3. Error bars reflect the standard error of the mean. Asterisks indicate significant performance benefits (faster RTs) for same-order versus different-order trials (* = $p \leq .01$, ** = $p \leq .001$). Left panel: RTs for task 1, right panel: RTs for task 2.

Figure 1:

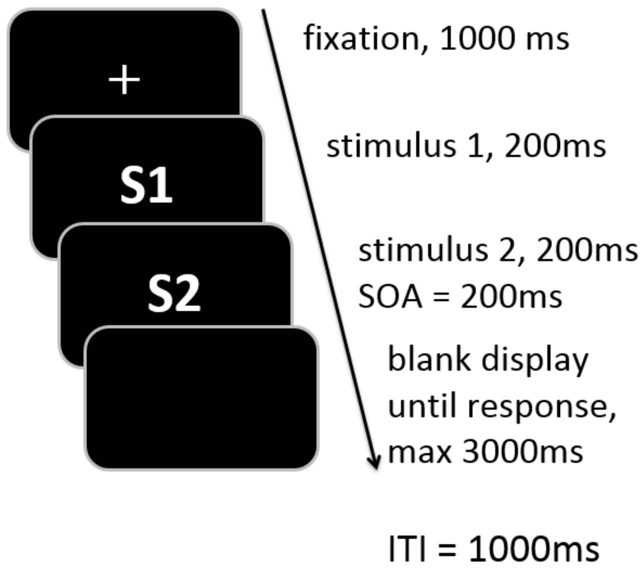
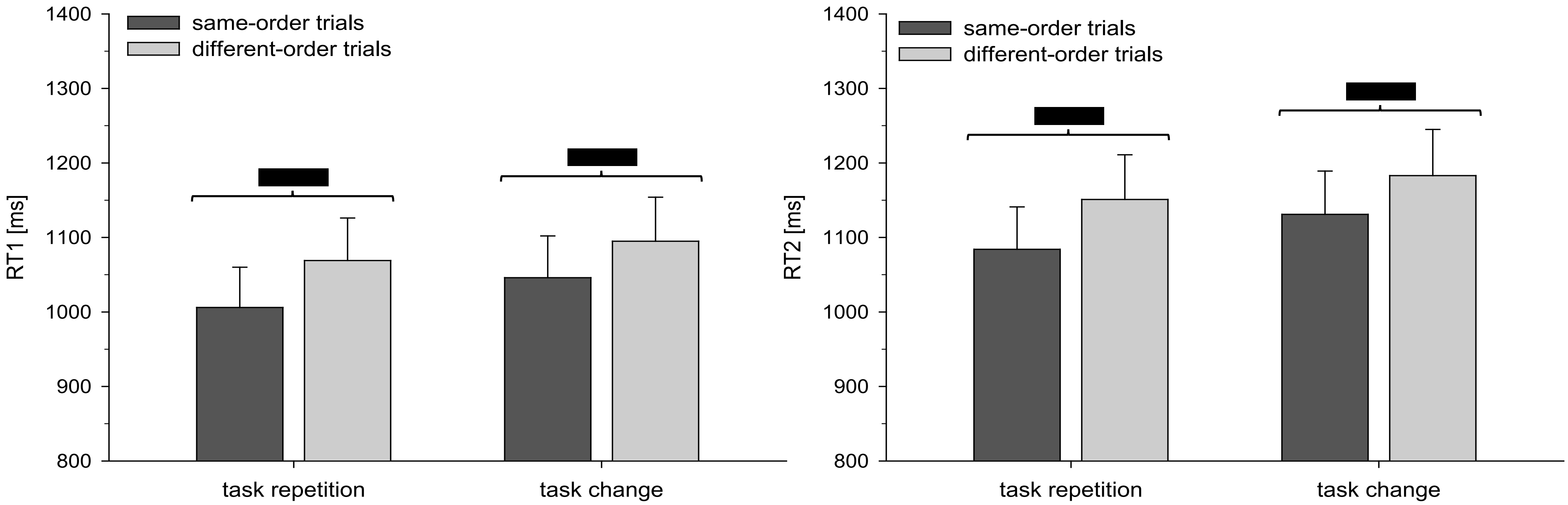

+
S1
S2
fixation, 1000 ms
stimulus 1, 200ms
stimulus 2, 200ms
SOA = 200ms
blank display
until response,
max 3000ms
ITI = 1000ms

Figure 2:


same-order trials
different-order trials
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800
task repetition
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same-order trials
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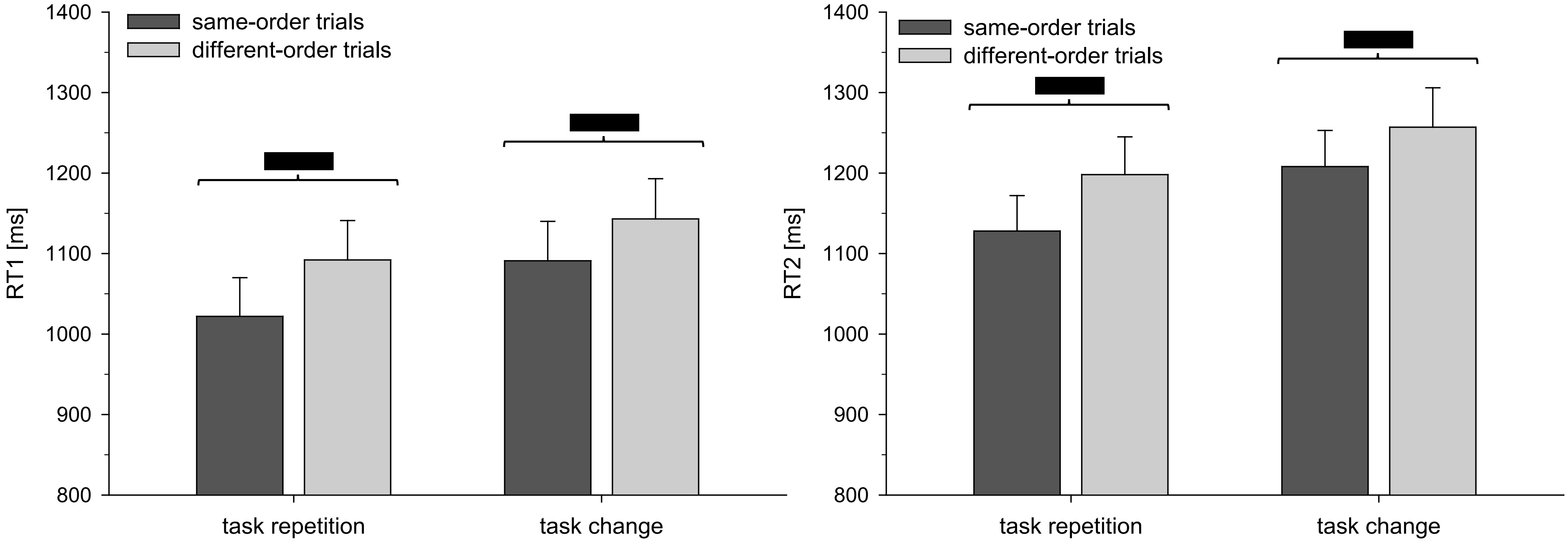
800
task repetition
task change

Figure 3: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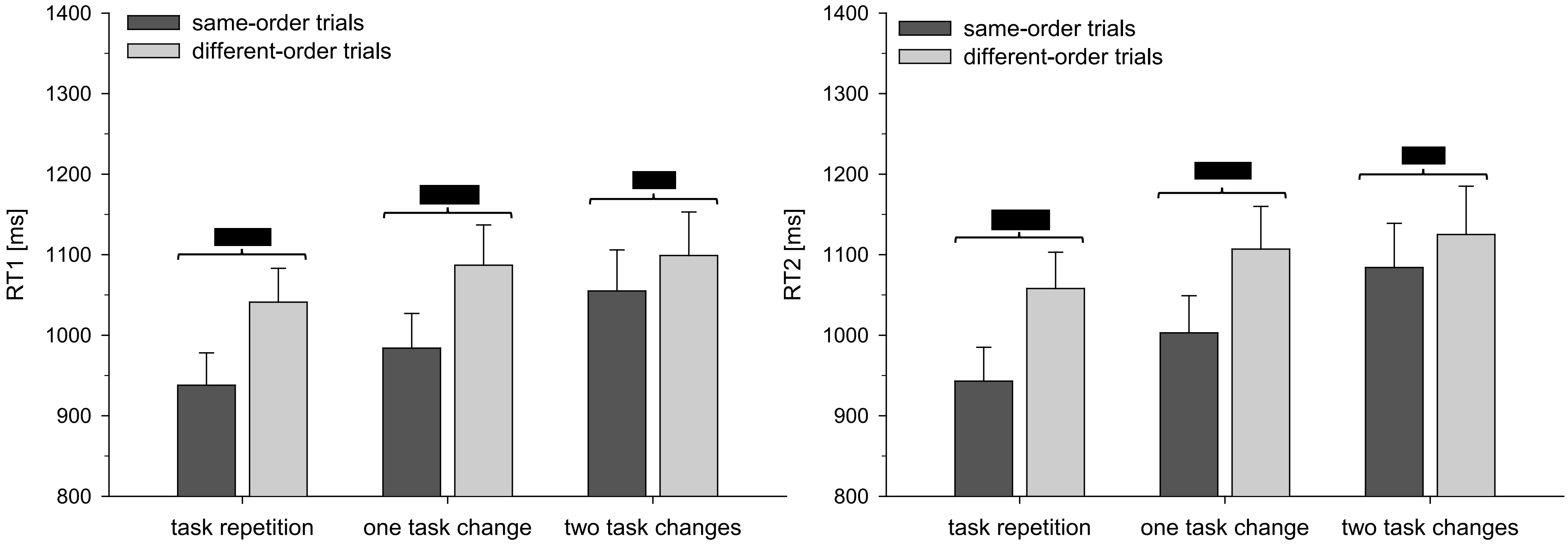
task repetition
task change

Figure 4: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RT1 [ms]
same-order trials
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task repetition
one task change
two task changes
1400
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1100
1000
900
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RT2 [ms]
same-order trials
different-order trials
task repetition
one task change
two task changes

The Causal Role of the Lateral Prefrontal Cortex for Task-order Coordination in Dual-task Situations: A Study with Transcranial Magnetic Stimulation

Sebastian Kübler[1,2], Alexander Soutschek[3], and Torsten Schubert[1,2]

Abstract

■ Dual tasks are characterized by the requirement for additional task-order coordination processes that schedule the processing order of two temporally overlapping tasks. Preliminary evidence from functional imaging studies suggests that lateral pFC (lPFC) activation correlates with implementing these task-order coordination processes. However, so far, it is unclear whether the lPFC is also causally involved in coordinating task order during dual-task performance and which exact mechanisms are implemented by this brain region. In this study, we addressed these open issues by applying online TMS during a dual-task situation. For this purpose, participants performed a dual task in fixed-order blocks with a constant order of tasks and in random-order block, in which the order of tasks varied randomly and thus demands on task-order coordination were increased. In Experiment 1, TMS of the lPFC compared with control TMS conditions impaired dual-task performance in random-order blocks, whereas performance in fixed-order blocks was unaffected by TMS. In Experiment 2, we tested for the specificity of the lPFC TMS effect on task-order coordination by applying TMS over the preSMA. We showed that preSMA TMS did not affect dual-task performance, neither in fixed-order nor in random-order blocks. Results of this study indicate that the lPFC, but not the preSMA, is causally involved in implementing task-order coordination processes in dual-task situations. ■

INTRODUCTION

In everyday life, we often perform two (or more) tasks simultaneously. Usually, in these multitasking situations severe performance decrements emerge compared with situations in which we perform the same tasks separately. This is shown in different dual-task (DT) paradigms, such as psychological refractory paradigm, in which participants perform two temporally overlapping choice RT tasks and which usually leads to increased processing times and/or error rates compared with single-task situations (Schubert, 1999; Pashler, 1994). The resulting DT costs can be explained by the assumption of a central bottleneck, which requires that central processing stages in the two tasks are processed serially (e.g., Pashler, 1994, and many others). Although the nature of the bottleneck is still a matter of debate and is subjected to structural and/or strategic reasons, we assume in line with other accounts (Schubert, 2008; Luria & Meiran, 2003, 2006; Sigman & Dehaene, 2006; Logan & Gordon, 2001; Meyer & Kieras, 1997; De Jong, 1995) that bottleneck processing requires additional cognitive control processes that schedule the serial processing order of the two tasks and temporally coordinate both task processing streams along the central bottleneck.

Studies employing the fMRI method (Stelzel, Kraft, Brandt, & Schubert, 2008; Szameitat, Lepsien, von Cramon, Sterr, & Schubert, 2006; Schubert & Szameitat, 2003; Szameitat, Schubert, Müller, & Von Cramon, 2002; D'Esposito et al., 1995) as well as lesion studies (Leclercq et al., 2000; Baddeley, Della Sala, Papagno, & Spinnler, 1997; McDowell, Whyte, & D'Esposito, 1997) give rise to the assumption that the lateral pFC (lPFC) plays a crucial role for implementing these task-order coordination processes. However, although the former can only provide correlational evidence for the association of a given brain region with a specific cognitive function (Logothetis, 2008), causal conclusions based on the latter are limited due to different restraints such as a lack in lesion focality (Rorden & Karnath, 2004). Therefore, even despite the evidence from neuroimaging and lesion studies, it is not yet clear whether or not the lPFC has a causal role for task-order coordination in DTs with overlapping task processing. Additionally and equally important, it is still an open question which exact processes are implemented by the lPFC to regulate the processing order of two temporally overlapping tasks. So far, different mechanisms of task-order coordination have been identified (Kübler, Reimer, Strobach, & Schubert, 2018) that operate on different timescales and subserve different

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functions for regulating task order (for an elaborate characterization, see Task-order Coordination and the lPFC section). However, it is not clear for which of those mechanisms the lPFC is recruited. In this study, we used transcranial magnetic stimulation (TMS), a noninvasive brain stimulation method, to address these open issues and to investigate the causal and functional role of the lPFC for task-order coordination in DTs.

Task-order Coordination and the lPFC

Evidence for the involvement of the lPFC in task-order coordination processes comes from neuroimaging studies applying DTs consisting of two temporally overlapping choice RT tasks. In an fMRI study by Szameitat et al. (2002; see also Stelzel et al., 2008), participants performed DT blocks in which they responded to an auditory and a visual stimulus that were presented one after the other with variable, that is, randomly changing, order. Importantly, participants were instructed to respond to both stimuli according to the order of their presentation. Neural activation was contrasted between these random-order blocks and fixed-order blocks, in which both tasks were presented with a constant stimulus order, for example, always the visual stimulus first and the auditory stimulus second. As a result, the authors found increased fMRI activation in a frontoparietal network, including the left lPFC with focal activation peaks close to the inferior frontal junction (IFJ) during random-order compared with fixed-order blocks. The IFJ is located at the intersection point of the precentral sulcus (PrCS) and the inferior frontal sulcus (IFS) and has also been shown to be consistently involved in other cognitive control tasks, such as the task switching, the Stroop, or the *n*-back paradigm (Brass, Derrfuss, Forstmann, & von Cramon, 2005; Derrfuss, Brass, Neumann, & von Cramon, 2005; Derrfuss, Brass, & von Cramon, 2004). In addition, on a behavioral level, the performance of DT trials with changing order compared with fixed order was accompanied by prolonged RTs for both tasks.

These results, that is, increased IFJ activation and prolonged RTs in random-order blocks compared with fixed-order blocks, are in line with the assumption that task-order coordination processes regulating the processing order are involved in DT blocks with varying task order but not in blocks with fixed order. In fixed-order blocks, as the order of tasks remains constant, participants can employ the same scheduling strategy for every trial throughout the entire block. Contrarily, in random-order blocks, the order of tasks varies unpredictably from trial to trial, and participants have to match the task processing order to the order of stimulus presentation. This requires that, across the entire block, participants need to monitor the order of stimuli and to schedule the processing order accordingly. According to several authors (Stelzel et al., 2008; Szameitat et al., 2002), the increased demands on task-order coordination processes during random-order compared with fixed-order blocks result in the additional recruitment of the IFJ, leading to increased neural activation.

Further evidence for the involvement of the lPFC in task-order coordination comes from a related line of research that investigates task coordination on a more fine-grained trial-by-trial level. In an event-related fMRI study, Szameitat et al. (2006) presented a DT consisting of an auditory and a visual choice RT task with varying stimulus order. The presentation of trials with randomly varying stimulus order allowed the authors for distinguishing between two different types of DT trials: same-order trials and different-order trials (see also Luria & Meiran, 2003, 2006; De Jong, 1995). In same-order trials, the task order was the same as compared with the previous trial, for example, in both trials the visual task was performed first and the auditory task was performed second. On the contrary, in different-order trials, the order of tasks was reversed relative to the preceding trial, for example, in the previous trial the visual task was performed first and the auditory task was performed second, but in the next trial the auditory task was performed first and the visual task was performed second. The authors analyzed fMRI activity for both trial types and found increased activation in regions of the left IFJ in different-order trials compared with same-order trials, which was accompanied by increased RTs in different-order compared with same-order trials.

According to Szameitat et al. (2006), trial-specific task-order coordination processes occur in different-order but not in same-order DT trials: In more detail, in different-order trials participants prepare the task order in the current DT trial according to a memory presentation of the task order in the previous trial. Because the stimulus order and thus task-order in different-order trials are reversed relative to the previous trial, participants need to overcome the prepared task order and to adapt the current task order to the correct stimulus order. This explains the additional processing demands in different-order compared with same-order trials and the related IFJ activation. Note that in same-order trials, participants can rely on the task order primed by the previous trial when performing the current trial because task order is repeated.

Although earlier fMRI studies provided rather correlative evidence for the involvement of the lPFC in task-order coordination, the first aim of the current study was to test for a potential causal role of the IFJ in task-order coordination by applying online TMS. Furthermore, we aimed to disentangle possible TMS effects on task-order coordination mechanisms, which can be revealed by a comparison of random-order and fixed-order blocks and task-order coordination mechanisms, which are observable when contrasting same-order and different-order trials. In addition, we wanted to test the specificity of potential TMS effects in the IFJ region by comparing these effects with those resulting from stimulating an alternative control region; as a candidate control region, we focused on the preSMA in Experiment 2, because the preSMA had recently been shown to be involved in bottleneck processing in DTs (Soutschek, Taylor, & Schubert, 2016).

TMS and Rationale of This Study

We applied online TMS because it allowed us to interfere with cortical information processing in narrowly circumscribed brain regions with high temporal resolution (Bestmann, 2008; Hallett, 2007; Pascual-Leone, Walsh, & Rothwell, 2000). By inducing an electrical field by means of electromagnetic induction, TMS can transiently and reversibly disturb cognitive functions implemented by the stimulated brain site and disrupt participants' task performance, which allows for causal inferences about the targeted brain region (Miniussi, Harris, & Ruzzoli, 2013; Siebner, Hartwigsen, Kassuba, & Rothwell, 2009). Recently, TMS had also been shown to provide reliable findings about different brain regions that are causally linked to the implementation of various cognitive control processes (Taylor, Nobre, & Rushworth, 2007; Chambers et al., 2006; Rushworth, Hadland, Paus, & Sipila, 2002).

In the current study, we presented participants with a DT consisting of an auditory and a visual choice RT task that overlapped in time and applied TMS. The DTs were presented in fixed-order and random-order blocks with the instruction to respond to the tasks in the order of stimulus presentation (Stelzel et al., 2008; Szameitat et al., 2002). Trials within the random-order blocks were further subdivided in same-order and different-order trials (Szameitat et al., 2006). We assessed DT performance under different conditions of TMS by measuring RTs and error rates, that is, the percentage of incorrect responses. Additionally, the accuracy of task-order coordination performance was assessed by analyzing the rates of task-order reversal trials. Note that, in task-order reversal trials, participants' task-order processing is reversed to the presented stimulus order, which reflects nonsuccessful task-order coordination.

We applied an order cue that informed participants about the order of stimuli in the upcoming trial. The logic behind presenting this order cue was to temporally separate task-order coordination from other mechanisms that may be involved in the processing of the DT (but not, specifically, in coordinating task order), such as perceptual or response selection processes (De Jong, 1995). We administered TMS after the presentation of the order cue and before the presentation of the first stimulus to exclusively interfere with task-order coordination but leave these other processes undisturbed. To control for effects of TMS on the cue processing (which might confound the impact of TMS on task-order coordination), we administered an additional control task. In this control task, participants were instructed to respond to the order of stimuli as it was indicated by the instructional order cue. Thus, instead of processing a DT in the correct order, participants were required to process the order cue and indicate its identity with corresponding button presses. As we only changed the instruction for participants in the control task but applied the same stimulus material, demands on visual processing of the order cue should be comparable between the DT and the control task. Hence, if TMS indeed interferes with cue processing, we should find impaired performance in the control task after stimulation. If, alternatively, TMS does not disturb performance in the control tasks, we can infer that stimulation has no effects on processing the order cue.

EXPERIMENT 1

In Experiment 1, we investigated the effects of IFJ TMS on task-order coordination relative to two control TMS conditions (no TMS and vertex TMS). We compared the effect of IFJ TMS on trials in fixed-order blocks and on same-order trials, as well as on different-order trials in random-order blocks. If the IFJ is causally linked to task-order coordination processes that are required in random-order blocks to adjust one's processing order to a changing stimulus order, TMS of the IFJ should result in decreased DT performance relative to control TMS conditions in both same-order and different-order trials; in other words, TMS of the IFJ should lead to slower RTs in random-order blocks compared with the control TMS conditions. In fixed-order blocks, however, demands on task-order coordination are reduced as participants can employ the same scheduling strategy on every single trial. Therefore, TMS of the IFJ should have no effect on the performance in fixed-order blocks. Alternatively, if the IFJ is causally involved in task-order coordination processes that are specific for different-order trials, that is, when the order of tasks changes relative to the preceding trial and memory-based preparation has to be overcome in the current trial, IFJ TMS should result in impaired DT performance compared with control TMS conditions only in different-order trials. In same-order trials and in trials of fixed-order blocks, there is no requirement to change the task order compared with the previous trial. Therefore, TMS of the IFJ should have no effect on the DT performance in these trials. To investigate whether any potential effects of IFJ TMS on task-order coordination are specific for this brain region, we conducted Experiment 2 in which we stimulated the preSMA, a brain region that has been recently shown to be involved in bottleneck processing (Soutschek et al., 2016).

Methods

Participants

Twenty healthy participants (12 women; mean age = 27.3 years, SD = 3.2 years) were invited to take part in the experiment after obtaining written informed consent. To determine an appropriate sample size, we conducted an a priori power analysis using the G*Power program (Faul, Erdfelder, Lang, & Buchner, 2007). For this analysis, we estimated a medium effect size of $f = 0.24$. With an α error probability of .05 and a power ($1 - \beta$ error probability) of .80, the analysis yielded a required sample size of $N = 15$. Note that this number of participants is similar to sample sizes in comparable studies using noninvasive brain stimulation methods in DT situation with temporally overlapping component tasks (Soutschek et al., 2016; Strobach, Soutschek, Antonenko, Floel, & Schubert, 2015). Bearing in mind that this a priori power analysis may underestimate the required sample size and to account for any potential dropout, we decided to invite 20 participants to guarantee sufficient power for analyses. Participants were paid 10 euros per hour for their participation. The experimental protocol conformed to the declaration of Helsinki as well as to common safety guidelines for TMS studies (Rossi, Hallett, Rossini, & Pascual-Leone, 2009). Approval of the local ethics committee was obtained before the commencement of the study. All participants were right-handed, were German native speakers, and had normal or corrected-to-normal vision. For four participants, neuronavigation (see below) failed as a result of technical problems. Those four participants' data could not be included in the analyses. Another participant reported adverse effects of TMS (rapid heartbeat) before commencing the control task (see below), and thus, only her data from the DT blocks could be used for analyses.

Apparatus and Stimuli

Participants performed a DT consisting of an auditory and a visual choice RT task (for a similar DT situation, see Stelzel et al., 2008). Stimuli for the auditory task consisted of three sine wave tones with a frequency of 250, 500, or 1000 Hz presented for 100 msec via headphones. Participants were instructed to respond to the low-, middle-, and high-pitched tones by pressing the keys "Y," "X," and "C" of a QWERTZ keyboard with the ring, middle, and index fingers of their left hand, respectively (note that, in contrast to the more common QWERTY keyboard, on the QWERTZ keyboard the response keys "Y," "X," and "C" are aligned next to each other from left to right). For the visual task, one of three digits ("1," "5," or "9") was presented centrally on a computer screen and subtended approximately 0.52° × 0.31° of the visual angle at a viewing distance of 80 cm. Visual stimuli remained visible for 100 msec, and participants responded to the digits in ascending order by pressing the keys "," (comma), "." (period), and "-" (dash) of a QWERTZ keyboard with the index, middle, and ring finger of their right hand, respectively.

Design and Procedure

Each DT trial started with the presentation of a central fixation cross (0.42° × 0.42°) for 1200 msec (Figure 1), followed by a centrally presented instructional order cue (0.52° × 0.31°), indicating the presentation order of both stimuli in the upcoming trial. This procedure was used so that participants execute task-order coordination processes before target presentation and, thus, temporally isolate them from other cognitive processes that are necessary for performing both component tasks (De Jong, 1995). The instructional cue lasted for 100 msec and was either the letter "T" or the letter "Z" in trials in which the tone task was presented first or the digit task was presented first, respectively. The cue target interval (CTI) was set to 600 msec. After the CTI, both target stimuli were presented for 100 msec each with a constant SOA of 200 msec. After the presentation of both target stimuli, the screen was cleared for a response period of 2900 msec, resulting in a total trial time of 5000 msec. Participants were instructed to react as fast and as accurately as possible to both stimuli according to the order of their presentation.

Participants performed the DT in two types of blocks. In fixed-order blocks, the presentation order of both stimuli remained constant throughout the entire block. On half of the fixed-order blocks, the auditory stimulus was presented first, on the other half of the fixed-order blocks, the visual stimulus was presented first. In random-order blocks, the presentation order of both stimuli varied randomly from trial to trial and unpredictably to participants. The instructional order cue was

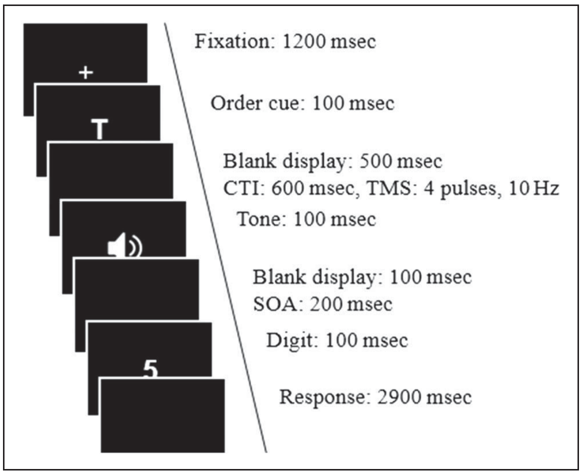

Figure 1. The time course for an exemplary DT trial in which the tone task was presented as the first task. Following a fixation cross an instructional order cue provides information of the presentation order of both stimuli in the upcoming trial. On TMS trials, four TMS pulses were administered with a frequency of 10 Hz during the CTI. After the CTI, both stimuli were presented for 100 msec with an SOA of 200 msec. The maximum time for both responses was set to 2900 msec.

presented in both block types. Fixed-order blocks consisted of 40 trials. Thirty-six of these trials were regular DT trials with two stimuli that required two responses. To discourage the usage of a response grouping strategy (Pashler & Johnston, 1989), the remaining 10% of the trials within fixed-order blocks were catch trials, in which the first task was omitted and only the second task was presented (Luria & Meiran, 2003). Random-order blocks consisted of 36 trials; in half of these trials the auditory stimulus was presented first, and in the other half the visual stimulus was presented first. Additionally, on 18 trials, the order of stimuli was repeated compared with the preceding trial, whereas on the remaining 18 trials, stimulus order was reversed relative to the previous trial.

The experimental session consisted of a practice phase and an experimental phase. The practice phase started with four single-task blocks, which were followed by two fixed-order and two random-order practice blocks. In the subsequent experimental phase, half of participants performed four fixed-order blocks and eight random-order blocks in the following sequence of blocks: two fixed-order blocks, four random-order blocks, two fixed-order blocks, and four random-order blocks. The remaining participants performed the same blocks in the reversed order. Subsequently, participants conducted a control task (see below).

TMS Procedure

TMS was applied with the eXimia Navigated Brain Stimulation System (Nexstim) using a focal bipulse figure-eight coil with a mean winding diameter of 50 mm and an outer winding diameter of 70 mm. Coil positioning over the IFJ was guided by neuronavigation software employing a Polaris Spectra 3D Optical tracking unit (NDI) that enables the recording of the real-time position and orientation of the TMS coil with respect to the participant's head with an accuracy below .035 mm. This procedure is based on a coil specific 3-D model, individual's structural MR images, and the stimulator parameters. Using this system, stimulation was applied to the IFJ, whereas the distance between the target area and the peak electric field was recorded for every TMS pulse. Structural T1 scans for each participant were acquired with a 3.0-T Siemens Magnetom Trio scanner using a 32-channel radiofrequency head coil beforehand.

TMS was administered in half of the trials of each block excluding catch trials and the first trial of each block. Stimulation was applied in trains of four pulses with a frequency of 10 Hz and an intensity of 110% of the individual's motor threshold (M = 37.6%), starting with cue offset and lasting for 300 msec (for a similar TMS protocol in a study on cued task switching, see Muhle-Karbe, Andres, & Brass, 2014). Stimulation was applied during the CTI to exclusively interfere with task-order coordination processes, but not with Task 1 or Task 2 processing. Note that with the last impulse delivered 200 msec before the presentation of the first stimulus and the perturbing effects of individual TMS pulses typically lasting for 80–120 msec (Miniussi et al., 2013; Bestmann, 2008), any TMS effects on stimulus processing, which should only occur after presentation of the first stimulus, are rather unlikely. Moreover, due to the short-lasting effect of online TMS (Rossi et al., 2009; Siebner et al., 2009) and the total trial duration of 5000 msec, carryover effects of stimulation on subsequent trials can be excluded. Coil position was varied blockwise, and TMS was applied either to the IFJ or to the vertex. In addition to trials without stimulation, vertex TMS was chosen as a second control condition to rule out that any observed effects may have been caused by confounding nonneural effects of TMS (Jung, Bungert, Bowtell, & Jackson, 2016). The IFJ TMS site was located at the junction between the IFS and the inferior part of PrCS based on the individual's structural brain scan (Derrfuss, Brass, von Cramon, Lohmann, & Amunts, 2009). The PrCS was defined as the first major sulcus anterior and running parallel to the central sulcus, and the IFS was defined as the first major sulcus located dorsal to the anterior ascending ramus of the Sylvian fissure and approximately running in a posterior–anterior direction. An average distance of 1.81 mm (SD = 1.04 mm) between the individual IFJ TMS site and the peak electric field was estimated throughout the entire experiment based on the real-time estimation of the electric field induced on the cortical surface by TMS. The vertex was located at the Pz electrode position according to the international 10–20 system. The coil was orientated in anterior direction perpendicular to the inferior prefrontal sulcus and the medial longitudinal fissure for the IFJ TMS site and the vertex, respectively, resulting in TMS pulses with a posterior–anterior initial current direction. Coil position between both target areas was changed after every second random-order block in a row of four random-order blocks, which guaranteed an equal distribution of TMS trials between both the IFJ TMS and vertex TMS conditions. Half of the participants started the experimental session with the TMS coil positioned over the IFJ TMS site, and the other half started the session with the coil positioned over the vertex.

Control Task

As TMS was applied after the presentation of the instructional order cue, potential effects of stimulation could, theoretically, also be explained by interference with the processing of the instructional order cue instead of disturbed task-order coordination. To exclude this confound, after finishing DT blocks, participants performed a control task to assess any effects of TMS on the processing of the instructional cue. For this purpose, participants were presented two random-order blocks consisting of 36 trials. Instead of responding to both target stimuli, participants were instructed to indicate the order of tasks as signaled by the instructional cue presented at the beginning of each trial. Participants responded to trials in which the tone task was presented first by pressing the "C" key with their left index finger and to trials on which the digit task was presented first by pressing the "," key with their right index finger. As in the DT blocks, a train of four TMS pulses with a frequency of 10 Hz and an intensity of 110% of the motor threshold was applied in half of the trials after the offset of the instructional cue. The coil position was changed after the first block, with half of the participants performing the first block with the coil positioned over the IFJ TMS site and the second block with the coil positioned over the vertex TMS site. For the other half of the participants, the order of the coil positioning was reversed.

Statistical Analysis

For the DT, we analyzed median RTs and error rates separately for the first task (Task 1, RT1) and the second task (Task 2, RT2). As an additional measure, we analyzed task-order reversal rates (trials on which participants' response order was reversed compared with stimulus order). For these analyses, trials with grouped (inter-response interval = RT2 − RT1 + SOA < 200; Miller & Ulrich, 2008) or omitted responses (M = 4.4%) were excluded from the data set and, exclusively for the RT analyses, trials with erroneous responses (M = 8.0%) and task-order reversals (M = 2.2%). For the control task, RTs and error rates were analyzed. ANOVAs and subsequent paired-sample t tests (two-tailed) as planned comparisons were calculated. A significance threshold of 5% was used for all analyses. The p values of the ANOVAs were adjusted according to the Greenhouse–Geisser correction when necessary.

Results

Task 1

The first analysis tested the effect of IFJ TMS on Task 1 performance in fixed-order, same-order, and different-order trials. Participants' median RT1 and error rates were analyzed using a 3 × 3 ANOVA with the within-subject factors Trial Type (fixed-order trials, same-order trials, different-order trials) and TMS (no TMS, vertex TMS, IFJ TMS). If our hypothesis holds true and IFJ TMS distinctively modulates performance on the different trial types, we should find a significant interaction of the factors trial type and TMS.

The significant main effect of the factor Trial Type, $F(2, 30) = 7.03$, $p < .01$, $\eta_p^2 = .32$, revealed an increase in RT1 from fixed-order trials (M = 811 msec) to different-order trials (M = 889 msec), $t(15) = 2.58$, $p = .02$, indicating the occurrence of task-order coordination processes. The significant main effect of TMS, $F(2, 30) = 5.62$, $p = .02$, $\eta_p^2 = .27$, revealed that TMS over the IFJ increased RT1 (M = 882 msec) compared with no TMS (M = 837 msec), $t(15) = 2.22$, $p = .04$, and vertex TMS (M = 819 msec), $t(15) = 2.55$, $p = .02$.

Most importantly, this TMS effect was modulated by the factor Trial Type, $F(4, 60) = 2.67$, $p = .04$, $\eta_p^2 = .15$, suggesting that IFJ TMS had distinctive effects in fixed-order, same-order, and different-order trials. Pairwise comparisons revealed no differences between TMS conditions for the fixed-order trials (all ps > .50; see Figure 2). For the same-order trials, IFJ TMS resulted in increased RT1 (M = 890 msec) relative to no TMS

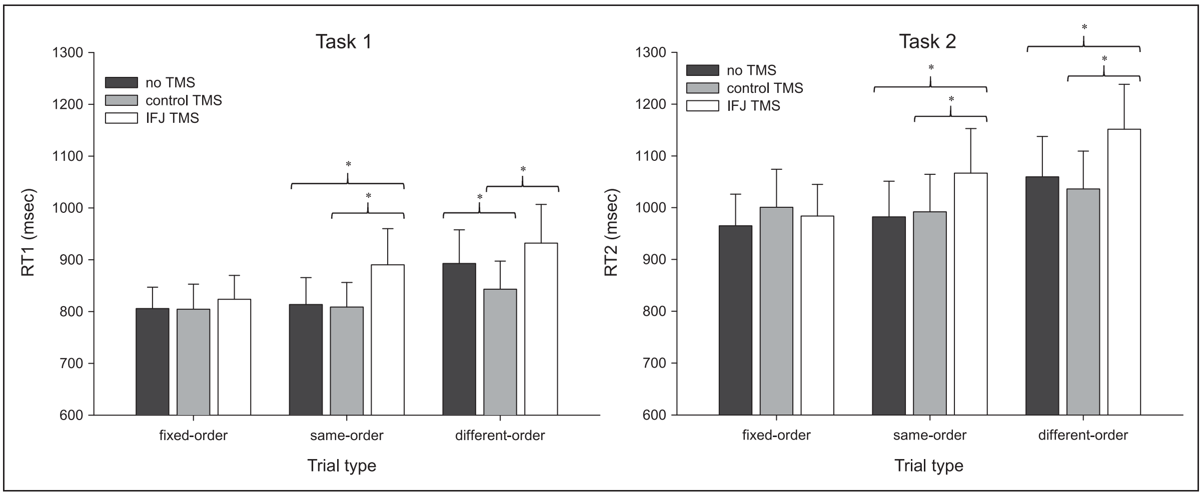

Figure 2. Mean RTs for Task 1 and Task 2 as a function of trial type and TMS conditions for Experiment 1. Error bars reflect the *SEM*. Asterisks indicate significant differences between TMS conditions. Left: RTs for Task 1, right: RTs for Task 2.

(M = 814 msec), $t(15) = 3.01$, $p < .01$, and vertex TMS (M = 809 msec), $t(15) = 2.92$, $p = .01$. Vertex and no TMS conditions did not differ, $t(15) = 0.35$, $p = .73$. A similar pattern was detected for different-order trials: RT1 after IFJ TMS (M = 932 msec) was, by trend, prolonged in comparison to no TMS (M = 893 msec), $t(15) = 1.77$, $p = .09$, and significantly slowed compared with vertex TMS (M = 843 msec), $t(15) = 2.85$, $p = .01$. Additionally, RT1 in different-order trials was approximately 49 msec faster after vertex TMS compared with no TMS, $t(15) = 2.30$, $p = .04$, which can be explained by unspecific TMS effects attributable to increased alertness due to acoustic stimulation or aversiveness (Marzi et al., 1998; Nikouline, Ruohonen, & Ilmoniemi, 1999).

For the error rate in Task 1, we did not find any significant effects of the factors trial type, $F(2, 30) = 1.34$, $p = .28$, $\eta_p^2 = .08$, and TMS, $F(2, 30) = 1.17$, $p = .32$, $\eta_p^2 = .07$, nor of their interaction, $F(4, 60) = 2.06$, $p = .10$, $\eta_p^2 = .12$ (for error rates, see Table 1).

Taken together, data of Task 1 showed that TMS of the IFJ results in impaired DT performance in random-order blocks, whereas it has no effect on trials in fixed-order blocks. This was indicated by significant differences in RT1 between the IFJ TMS condition and both control TMS conditions in same-order trials, as well as a significant RT difference in different-order trials after IFJ TMS compared with control TMS and a respective trend for the comparison between IFJ TMS and no TMS.

Task 2

To test whether IFJ TMS also disrupts performance on Task 2, we analyzed participants' median RT2 and error rates using the same 3 × 3 ANOVA as for the analysis of the Task 1 data. Again, the significant effect of Trial Type on RT2, $F(2, 30) = 7.35$, $p < .01$, $\eta_p^2 = .33$, indicated the occurrence of task-order coordination processes. Furthermore, we found a significant main effect of TMS, $F(2, 30) = 6.09$, $p = .02$, $\eta_p^2 = .29$, indicating increased RT2 after IFJ TMS (M = 1067 msec) compared with no TMS (M = 1002 msec), $t(15) = 2.88$, $p = .01$, and vertex TMS (M = 1010 msec), $t(15) = 2.34$, $p = .02$.

More importantly and similar to RT1, the significant interaction Trial Type × TMS, $F(4, 60) = 4.14$, $p = .02$, $\eta_p^2 = .21$, suggested that TMS effects differed between the different trial types. In fixed-order trials, IFJ TMS did not increase RT2 compared with both control TMS conditions (both ps > .55). In same-order trials, RT2 was prolonged after IFJ TMS (M = 1067 msec) compared with no TMS (M = 982), $t(15) = 2.95$, $p = .01$, and vertex TMS (M = 993 msec), $t(15) = 3.33$, $p < .01$. No difference was found between both the no TMS and vertex TMS conditions, $t(15) = 0.63$, $p = .54$. Also in different-order trials, IFJ TMS resulted in increased RT2 (M = 1151 msec) compared with no TMS (M = 1059 msec), $t(15) = 4.12$, $p = .001$, and vertex TMS (M = 1036 msec), $t(15) = 2.91$, $p = .01$, whereas the latter two conditions did not differ, $t(15) = 0.75$, $p = .46$.

Regarding error data in Task 2, we found a significant main effect of the factor TMS, $F(2, 30) = 4.43$, $p = .02$, $\eta_p^2 = .23$, indicating an unspecific TMS effect (see Table 1): For all trial types, the error rate was reduced in the no TMS condition (M = 4.46%) compared with the IFJ TMS condition (M = 6.60%), $t(15) = 2.65$, $p = .02$, and the vertex TMS condition (M = 5.73%), $t(15) = 2.25$, $p = .04$. Both, the IFJ and vertex TMS conditions did not differ, $t(15) = 1.12$, $p = .28$. The effect of Trial Type, $F(2, 30) = 2.19$, $p = .13$, $\eta_p^2 = .13$, and the interaction of Trial Type × TMS, $F(4, 60) = 0.28$, $p = .89$, $\eta_p^2 = .02$, did not reach significance.

In summary and similar to Task 1, data from Task 2 show that TMS of the IFJ results in disrupted DT

Table 1. Mean Rates of Errors for Task 1 and Task 2 in Percentage (and Standard Deviation) from Experiment 1 and Experiment 2 as a Function of Trial Type and TMS Condition

	Trial Type					
	Fixed-order Trial		*Same-order Trial*		*Different-order Trial*	
TMS Condition	*Task 1*	*Task 2*	*Task 1*	*Task 2*	*Task 1*	*Task 2*
Experiment 1						
No TMS	2.43% (2.46%)	3.65% (3.72%)	3.47% (2.78%)	4.43% (3.94%)	5.38% (4.36%)	5.30% (4.34%)
Control TMS	4.51% (6.72%)	4.86% (4.81%)	3.82% (3.19%)	5.73% (5.78%)	3.13% (4.04%)	6.60% (6.24%)
IFJ TMS	3.13% (3.35%)	4.86% (3.72%)	6.08% (5.85%)	7.29% (7.58%)	5.20% (6.49%)	7.64% (6.37%)
Experiment 2						
No TMS	3.70% (6.03%)	5.86% (8.04%)	3.70% (4.07%)	6.02% (6.52%)	9.03% (7.52%)	9.34% (7.38%)
Control TMS	3.40% (3.51%)	4.01% (4.49%)	6.17% (7.89%)	7.25% (5.88%)	6.64% (6.95%)	8.03% (6.73%)
preSMA TMS	5.71% (11.45%)	7.87% (16.09%)	4.17% (7.40%)	7.56% (9.13%)	6.48% (6.94%)	10.49% (10.85%)

performance in same-order and different-order trials compared with control TMS conditions. In fixed-order trials, however, IFJ TMS does not affect DT performance.

Task-order Reversals

We analyzed task-order reversal rates as an additional measure for task-order coordination processes. On average, participants responded to both tasks in a reversed order compared with stimulus presentation on 2.20% of all trials. The percentage of task-order reversals was, thus, rather low (see Table 2), most probably due the usage of the instructional order cue at the beginning of each trial. Despite this low rate of task-order reversals and the thereby reduced statistical power, which should be kept in mind as caveat, we conducted a 3 × 3 ANOVA with the within-subject factors Trial Type (fixed-order trials, same-order trials, different-order trials) and TMS (no TMS, vertex TMS, IFJ TMS). This ANOVA revealed a significant main effect of the factor Trial Type, $F(2, 30) = 7.93$, $p < .01$, $\eta_p^2 = .35$, indicating that participants adhered to the correct task order with different success in different trial conditions: Task-order reversal rate increased from fixed-order trials ($M = 0.55\%$) to same-order trials ($M = 2.23\%$), $t(15) = 2.03$, $p = .03$, and from same-order trials to different-order trials ($M = 4.05\%$), $t(15) = 2.22$, $p = .02$. The effect of the factor TMS was not significant, $F(2, 30) = 1.97$, $p = .16$, $\eta_p^2 = .12$.

However, there was a tendency for a significant interaction between the factors trial type and TMS, $F(4, 60) = 2.27$, $p = .07$, $\eta_p^2 = .13$. A closer inspection of the data revealed that this interaction is mostly driven by the fact that in same-order trials participants produced more task-order reversals after IFJ TMS ($M = 3.30\%$) relative to the vertex TMS condition ($M = 1.39\%$), $t(15) = 2.55$, $p = .02$. There was also a tendency for an increased task-order reversal rate compared with the no TMS condition ($M = 2.00\%$); however, this comparison just failed significance, $t(15) = 1.89$, $p = .07$. In different-order trials, task-order reversal rate was significantly increased after IFJ TMS ($M = 4.69\%$) compared with no TMS ($M = 3.13\%$), $t(15) = 2.26$ $p = .04$, and numerically higher after IFJ TMS compared with vertex TMS ($M = 4.34\%$); however, this trend failed to pass the statistical threshold, $t(15) = 1.18$, $p = .25$. On fixed-order trials, IFJ TMS did not increase task-order reversal rates compared with control conditions.

Control Task

To control for a possible confounding influence of TMS on cue processing, we analyzed participants' performance in the control task, which was conducted after the DT blocks. For that purpose, we analyzed participants' median RTs by using a 2 × 3 ANOVA with the within-subjects factors Trial Type (same-order trials, different-order trials) and TMS (no TMS, vertex TMS, IFJ TMS). The main effect of Trial Type, $F(1, 14) < 1$, $p = .43$, $\eta_p^2 = .05$, did not reach significance, neither did the main effect of TMS, $F(2, 30) < 1$, $p = .80$, $\eta_p^2 = .02$, nor the interaction between Trial Type and TMS, $F(4, 60) < 1$, $p = .67$, $\eta_p^2 = .03$. A similar pattern was found for the error data. A 2 × 3 ANOVA with the within-subject factors Trial Type (same-order trials, different-order trials) and TMS (no TMS, vertex TMS, IFJ TMS) did not reveal a significant effect of Trial Type, $F(1, 14) < 1$, $p = .84$, $\eta_p^2 < .01$, TMS, $F(2, 28) < 1$, $p = .46$, $\eta_p^2 = .05$, nor Trial Type × TMS, $F(2, 28) = 2.28$, $p = .12$, $\eta_p^2 = .14$. Altogether, RT and error data of the control task provide no evidence for significant effects of IFJ TMS on the processing of the instructional cue. RT and error data of the control task can be found in Table 3.

Table 2. Mean Rates of Task-order Reversals in Percentage (and Standard Deviation) from Experiment 1 and Experiment 2 as a Function of Trial Type and TMS Condition

	Trial Type		
TMS Condition	*Fixed-order Trial*	*Same-order Trial*	*Different-order Trial*
Experiment 1			
No TMS	0.78% (1.34%)	2.00% (3.73%)	3.13% (3.92%)
Control TMS	0.69% (1.90%)	1.39% (2.59%)	4.34% (5.27%)
IFJ TMS	0.17% (0.70%)	3.30% (4.67%)	4.69% (5.14%)
Experiment 2			
No TMS	0.15% (0.45%)	0.62% (0.98%)	3.63% (2.86%)
Control TMS	0.15% (0.66%)	1.23% (1.71)	2.01% (2.49%)
preSMA TMS	0.46% (1.07%)	0.62% (1.52%)	2.01% (3.41%)

Table 3. Mean RTs in Milliseconds and Mean Error Rates in Percentage (and Standard Deviation) of the Control Task from Experiment 1 and Experiment 2 for Each, Depending on the TMS Condition

	Trial Type			
	Same-order Trial		*Different-order Trial*	
TMS Condition	*RT (msec)*	*Error Rate (%)*	*RT (msec)*	*Error Rate (%)*
Experiment 1				
No TMS	509 (104)	5.86 (6.99)	502 (127)	4.01 (5.97)
Control TMS	516 (133)	4.32 (8.64)	506.24 (135)	5.56 (6.87)
IFJ TMS	503 (90)	4.94 (7.83)	507.93 (98)	2.47 (4.75)
Experiment 2				
No TMS	553 (275)	1.17 (2.33)	541 (253)	3.80 (3.23)
Control TMS	540 (224)	5.26 (10.71)	538 (235)	4.90 (8.45)
preSMA TMS	539 (221)	5.26 (8.58)	523 (245)	4.09 (6.64)

Discussion

Experiment 1 revealed that TMS over the IFJ results in impaired performance in DT situations with random task order: RTs in both component tasks were increased after IFJ TMS relative to control conditions in same-order and different-order trials, whereas RTs in trials in fixed-order blocks were unaffected by the TMS manipulation. Note that even for the case of Task 1 in different-order trials, RT1 was numerically and by trend larger for the IFJ TMS condition compared with the no TMS condition.[1] These results are in line with the assumption that the IFJ is causally involved in the implementation of task-order coordination processes in random-order blocks. More specifically, the IFJ seems to be recruited for implementing coordination processes that are necessary in DTs with random task-order, such as matching the processing order to a constantly changing normative task order (Stelzel et al., 2008; Szameitat et al., 2002). Importantly, we found an effect of IFJ TMS in both same-order and different-order trials, suggesting that the IFJ is not involved in implementing task-order processes that are specific for different-order trials, that is, when the order of tasks changes relative to the preceding trial.

Two potential counterargumentations need to be addressed. First, one might argue that the reported effects can also be explained by impaired processing of the task stimuli due to TMS rather than interference with task-order coordination. However, this argumentation is rather unlikely, as the effects of individual TMS pulses typically last for 80–120 msec (Miniussi et al., 2013; Bestmann, 2008), and in our paradigm, the time interval between the last TMS impulse and stimulus presentation lasted 200 msec. Furthermore, if TMS had interfered with stimulus processing, we should have also found TMS effects in fixed-order blocks because demands on stimulus processing do not differ between both block types. As this was not the case, we conclude that TMS of the IFJ interfered with task-order coordination and not with the processing of the target stimuli. According to a second argumentation, TMS of the IFJ might have, theoretically, impaired the processing of the order cue rather than task-order coordination. However, we demonstrated that TMS did not affect performance in a control task, in which participants had to indicate the order of stimuli as it was indicated by the instructional order cue. This result suggests that TMS of the IFJ did not interfere with the cue processing. Nevertheless, one might, theoretically, argue that demands on cue stimulus processing and stimulus identification might differ between the DT and the control task due to different instructions and responses on the presented order cue in the DT and the analog stimulus in the control task. However, we used the same stimulus material in both task situations, and earlier studies suggested that manipulations of stimulus material (e.g., stimulus degradation: Frowein & Sanders, 1978; stimulus contrast: Pachella & Fisher, 1969; similarity between stimuli: Shwartz, Pomerantz, & Egeth, 1977) rather than task instructions (Spijkers & Walter, 1985; Sanders, 1980) affect the early processing stages during the perception of visual stimuli (for an overview, see Sanders, 1990). Thus, as the stimulus material was physically identical for both tasks, we assume that that the demands on stimulus cue identification are similar across the DT and the control task situations. Consequently, there would be no reason to assume that TMS has affected stimulus processing in the one but not the other situation.

EXPERIMENT 2

The aim of Experiment 2 was to test whether or not the causal function for task-order coordination as shown in

Experiment 1 is specific for the IFJ. For that purpose, during a DT with fixed-order and random-order of the component tasks, we applied TMS over the preSMA, which has recently been associated with bottleneck processing (Soutschek et al., 2016). The preSMA was chosen as the site of stimulation because this brain region, in addition to the IFJ, shows increased neural activation during DTs with variable task-order (Szameitat et al., 2002; see also Schubert & Szameitat, 2003) and evidence from single-cell and lesion studies in primates (Shima & Tanji, 1998; Tanji & Shima, 1994) as well as from neurophysiological studies on humans (Nachev, Kennard, & Husain, 2008; Kennerley, Sakai, & Rushworth, 2004) suggests that the preSMA is pivotal for the sequencing of multiple motor actions and the updating of complex motor plans. Applying TMS on the preSMA in a DT with the same procedure and protocol as in Experiment 1 allows us to investigate if the IFJ should be regarded as a specific region for task-order coordination or if also other brain regions are causally related to task-order coordination.

Methods

Participants

In analogy to Experiment 1, 20 healthy participants (13 women; mean age = 23.6 years, SD = 3.9 years) were invited to take part in the experiment after obtaining written informed consent. All participants were right handed, were German native speakers, and had normal or corrected-to-normal vision. One participant was excluded from analysis due to poor DT performance (less than 50% of correct trials). For another participant, neuronavigation could not be performed reliably. Data of the remaining 18 participants were included in analyses.

Apparatus and Stimuli

Apparatus and stimuli were the same as in Experiment 1.

Design and Procedure

The procedure and design were the same as in Experiment 1.

TMS Procedure

The TMS procedure was similar to the one employed in Experiment 1, with four TMS pulses applied during the CTI with an intensity of 110% of the individual's motor threshold (M = 39.9%) and a frequency of 10 Hz. TMS was applied either to preSMA or to the vertex to test whether the preSMA is causally involved in task-order coordination processes. As in Experiment 1, a neuronavigated approach was used to validate coil position over the preSMA in real time throughout the entire experiment. Again, structural brain scans for each participant were acquired beforehand. The preSMA TMS site was located on the midline, approximately 1 cm anterior to intersection of the verticofrontal line and the outer cortex surface (Mayka, Corcos, Leurgans, & Vaillancourt, 2006; Muessgens, Thirugnanasambandam, Shitara, Popa, & Hallett, 2016). An average distance of 1.00 mm (SD = 0.55 mm) between the individual preSMA TMS site and the peak electric field was estimated throughout the whole experiment. As in Experiment 1, the vertex was located at the Pz electrode position according to the international 10–20 system. The average distance between the vertex and preSMA TMS site amounted to 48.46 mm (SD = 8.14 mm). The coil was orientated in anterior direction perpendicular to the medial longitudinal fissure for both the preSMA TMS site and the vertex, respectively.

Statistical Analysis

Statistical analyses were similar to those in Experiment 1. For these analyses, trials with omitted or grouped responses (M = 8.1%) were excluded from the data set and, exclusively for the RT analyses, task-order reversal trials (M = 1.2%) and trials with erroneous responses (M = 9.0%).

Results

Task 1

In the first analysis, we tested whether the preSMA is recruited for the implementation of task-order coordination processes in the current DT situation. Participants' median RT1 and error rate were analyzed with a 3 × 3 ANOVA with the within-subject factors Trial Type (fixed-order trials, same-order trials, different-order trials) and TMS (no TMS, vertex TMS, preSMA TMS). Most importantly, the analysis of RT1 revealed no effect of the factor TMS, $F(2, 34) = 0.54$, $p = .56$, $\eta_p^2 = .03$, nor of its interaction with the factor Trial Type, $F(4, 68) = 0.52$, $p = .55$, $\eta_p^2 = .03$, suggesting that preSMA TMS had neither a general nor a trial-specific effect on DT performance. The only significant effect was found for the factor Trial Type, $F(2, 34) = 9.33$, $p = .001$, $\eta_p^2 = .35$, indicating the occurrence of task-order coordination processes (see Table 4).

The analysis of error rate in Task 1 (Table 1) did not reveal any significant effects, neither of the factor Trial Type, $F(2, 34) = 2.43$, $p = .10$, $\eta_p^2 = .13$, TMS, $F(2, 34) < .01$, $p = 1.00$, $\eta_p^2 = .00$, nor of their interaction Trial Type × TMS, $F(4, 68) = 1.70$, $p = .16$, $\eta_p^2 = .09$. In summary, Task 1 data provided no evidence that preSMA TMS modulates DT performance.

Task 2

As for RT1, we analyzed RT2 and error rates using a 3 × 3 ANOVA with the within-subject factors Trial Type (fixed-order trials, same-order trials, different-order trials)

Table 4. Mean RTs in Milliseconds (and Standard Deviation) for Task 1 and Task 2 as a Function of Trial Type and TMS Conditions for Experiment 2

	Trial Type		
TMS Condition	*Fixed-order Trial*	*Same-order Trial*	*Different-order Trial*
Task 1			
No TMS	861 (259)	851 (301)	967 (420)
Control TMS	852 (254)	903 (375)	937 (328)
preSMA TMS	872 (268)	872 (310)	975 (367)
Task 2			
No TMS	992 (274)	994 (320)	1100 (373)
Control TMS	994 (291)	1075 (455)	1165 (410)
preSMA TMS	1045 (334)	1029 (363)	1130 (405)

and TMS (no TMS, vertex TMS, preSMA TMS). In this analysis, neither the factor TMS, $F(2, 34) = 1.60$, $p = .22$, $\eta_p^2 = .09$, nor the interaction Trial Type × TMS, $F(2, 34) = 1.22$, $p = .31$, $\eta_p^2 = .07$, was significant. Again, only the effect of the factor Trial Type was significant, $F(2, 34) = 11.92$, $p < .001$, $\eta_p^2 = .41$, indicating the occurrence of task-order coordination.

Regarding the error rate in Task 2, the effect of the factors Trial Type, $F(2, 34) = 1.41$, $p = .26$, $\eta_p^2 = .08$, as well as TMS, $F(2, 34) = 1.20$, $p = .31$, $\eta_p^2 = .07$, were not significant. Neither was the effect of their interaction, $F(2, 34) = 0.43$, $p = .79$, $\eta_p^2 = .03$. To summarize, similar to data on Task 1, we found no significant effects of preSMA TMS on DT performance in Task 2.

Task-order Reversals

Similar to Experiment 1, task-order reversal rates were rather low ($M = 1.17\%$, see Table 2). To test for effects of preSMA TMS on DT accuracy, task-order reversal rates were analyzed using a 3 × 3 ANOVA with the within-subject factors trial type (fixed-order trials, same-order trials, different-order trials) and TMS (no TMS, vertex TMS, preSMA TMS). We found a significant main effect of the factor Trial Type, $F(2, 34) = 19.10$, $p < .001$, $\eta_p^2 = .53$. The task-order reversal rate increased from fixed-order trials ($M = 0.26\%$) over same-order trials (0.82%), $t(17) = 2.50$, $p = .02$, to different-order trials ($M = 2.54\%$), $t(17) = 4.32$, $p < .001$, indicating that participants were able to adjust their task order to the stimulus order with different success across different trial types. The factor TMS did not reach significance, $F(2, 34) = 0.70$, $p = .51$, $\eta_p^2 = .04$. The significant interaction of Trial Type and TMS, $F(4, 68) = 3.29$, $p = .02$, $\eta_p^2 = .16$), suggested different effects of TMS on task-order reversals for each trial type. However, this significant interaction was mainly driven by an unspecific TMS effect in different-order trials. Compared with no TMS ($M = 3.63\%$), task-order reversal rate was reduced after preSMA TMS ($M = 2.01\%$), $t(17) = 2.15$, $p = .05$, and by trend after vertex TMS ($M = 2.01\%$), $t(17) = 1.98$, $p = .06$. Both TMS conditions did not differ significantly, $t(17) = 0.00$, $p = 1.00$. No other pairwise comparison revealed significant differences (all ps > .10). In summary, preSMA TMS compared with vertex and no TMS did not modulate DT accuracy as measured by task-order reversal rates.

Control Task

Median RTs as well as error rates of the control task were analyzed employing a 2 × 3 ANOVA similar to the related analysis in Experiment 1. For both RTs and error rates (see Table 3), these analyses revealed no significant effects (all ps > .27), suggesting that preSMA TMS did not affect the processing of the instructional order cue.

Comparison across Experiments

We demonstrated that TMS of the preSMA compared with control conditions does not modulate DT performance at all. Therefore, to assess whether this result pattern can be distinguished from the IFJ TMS result pattern of Experiment 1, we conducted a between-experiment analysis. For that purpose, we calculated a repeated-measures ANOVA with the within-subject factors Task (Task 1, Task 2), Trial Type (fixed-order trials, same-order trials, different-order trials), and TMS (no TMS, vertex TMS, TMS) as well as Experiment (Experiment 1, Experiment 2) as a between-subject factor on participants' median RTs from Experiments 1 and 2. Most importantly, this analysis revealed a significant interaction of the factors Trial Type, TMS, and Experiment, $F(4, 128) = 2.66$, $p = .036$, $\eta_p^2 = .08$, indicating that the effect of TMS on

RTs in same-order trials and different-order trials differed between both experiments. Thus, we can confirm that, although we found an effect of the IFJ TMS on DT performance in same-order and different-order trials in Experiment 1, we did not find similar effects in Experiment 2 when applying preSMA TMS. No other interaction including the between-subject factor Experiment was significant (all ps $> .10$).

Discussion

In Experiment 2, we showed that stimulation of the preSMA has no effect on the implementation of task-order coordination: Compared with IFJ TMS, preSMA TMS did not affect DT performance, neither in trials of fixed-order blocks nor in same-order and different-order trials of random-order blocks. Together with the findings of a subsequent cross-experiment analysis, these results indicate the specificity of the TMS effects for the IFJ compared with other brain regions and emphasize its dominant role for coordinating task order in DT situations.

GENERAL DISCUSSION

The aim of this study was to investigate the causal and functional role of the IFJ for implementing task-order coordination processes in DT situations. For this purpose, we applied online TMS during a DT with fixed and random order of the component tasks. In Experiment 1, TMS of the IFJ compared with control TMS conditions resulted in impaired DT performance in same-order and different-order trials in random-order blocks as reflected in increased RTs for Task 1 and Task 2. Performance in trials in fixed-order blocks was unaffected by IFJ stimulation. Additionally, in a control task, we showed that stimulation did not affect cue identification, suggesting that the TMS effects on DT performance are most probably not attributable to interference with cue processing due to TMS. In Experiment 2, we showed that preSMA TMS had no effect on DT performance, neither in fixed-order nor in random-order blocks. This pattern of results was confirmed in a combined analysis of both experiments emphasizing the specific role of the IFJ for task-order coordination. In summary, the data of both experiments are in line with the assumption that the IFJ is causally involved in implementing task-order coordination processes in DT situations with varying order of the component tasks.

Prior evidence for the potential involvement of the IFJ in implementing task-order coordination in DTs stems from fMRI studies (Stelzel et al., 2008; Szameitat et al., 2002, 2006) as well as studies testing neurological patients suffering from brain damage (Baddeley et al., 1997; McDowell et al., 1997). However, based on the fMRI method, only correlational conclusions about the IFJ and its role for task-order coordination can be drawn (Logothetis, 2008). Furthermore, evidence from lesion studies can only be interpreted with caution. Because brain lesions are usually not restricted to a narrowly circumscribed brain region (like the IFJ) but instead affect vast cortical as well as subcortical areas (Rorden & Karnath, 2004), impairments in task-order coordination in neurological populations can also point to a potential influence of brain damage beyond the IFJ. As TMS is characterized by high spatial (and temporal) resolution and as it is able to interfere with neural information processing, the current study allows for overcoming these issues by providing evidence that the IFJ is indeed causally involved in the execution of task-order coordination.

In addition to testing its causal role, a further aim of the study was to specify the functional contribution of the IFJ for task-order coordination. We showed that stimulation of the IFJ impaired DT performance in trials in random-order blocks, that is, in same-order and different-order trials, but not in trials in fixed-order blocks. Thus, based on our results, we conclude that the IFJ is recruited for task-order control processes that are necessary to adjust the processing order of the to-be-performed tasks to the permanently varying stimulus order: In random-order blocks, participants are instructed to respond to both tasks according to the order of stimulus presentation. As a result, in each trial, participants have to judge the order of stimuli based on their temporal onsets and then adjust the processing order of the respective tasks accordingly. But how, exactly, can this adjustment be realized? According to cognitive models on task-order coordination (Kübler et al., 2018; Luria & Meiran, 2003, 2006), task-order in DT situations is regulated by a higher order control structure, the task-order set, which contains information about the specific processing order of the two tasks. When performing a DT trial, participants have to monitor the order of stimuli and implement the appropriate order set, that is, the order set that matches the stimulus order, which then schedules the processing of the component tasks. Note that if participants would not use this higher order representation and, instead, would simply rely on control processes on the level of the component tasks, we should find a performance benefit for different-order trials compared with trials in which the order is repeated relative to the previous trial (i.e., same-order trials). This is so, as in different-order trials, the first task of the current trial was the second task in the preceding trial resulting in a local task repetition despite a global change in task order. However, the finding that performance in different-order trials is impaired compared with same-order trials indicates that participants indeed take global order information, as it is represented by the task-order set, into account when performing DTs with variable order. The current results suggest that the IFJ is relevant for selecting and activating the appropriate task-order set in random-order blocks, when participants have to switch between different task-orders. As a result, TMS of the IFJ results in impaired DT performance in this block type. In fixed-order blocks, when participants know the task order in each trial in advance, there is no additional need for the IFJ to implement these task-order coordination processes, and thus, TMS has no effect on DT performance.

As an alternative, one could argue that the IFJ is causally involved in the monitoring of and the decision about the stimulus order rather than implementing the appropriate order set accordingly. Note that in the applied paradigm information about the stimulus order was given by the instructional order cue at the beginning of each trial. Importantly, however, TMS of the IFJ had no effect on the performance in the control task. In this control task, participants were instructed to respond to the order of stimuli (as it was indicated by the instructional order cue). Thus, instead of responding to both target stimuli in a specific order, participants were only required to monitor the information given by the instructional cue and to make a decision about the stimulus order. Consequently, in both tasks participants had to employ the same monitoring processes for retrieving the information given by the instructional order cue. Importantly, as IFJ TMS did not affect performance in this control task, it is rather unlikely that the IFJ is recruited for monitoring and decisional processes with respect to the stimulus order.

The current findings are in line with findings of recent fMRI studies suggesting that the IFJ plays a crucial role for the implementation of cognitive control in different experimental paradigms (Muhle-Karbe et al., 2016; Brass et al., 2005; Derrfuss et al., 2004, 2005). For example, in their fMRI study on task switching, Braver, Reynolds, and Donaldson (2003) contrasted single-task blocks, in which participants only performed one of two choice RT tasks, with mixed blocks, in which participants had to constantly switch between two different choice RT tasks. As a result, they reported focal activation peaks within the lPFC closely located to the IFJ during trials in mixed blocks compared with trials in single-task blocks, which led them to conclude that the IFJ plays an important role for the selection and representation of specific task information (i.e., the task set) during task switching. Similar results have also been found for other cognitive control tasks such as the Stroop paradigm (for an overview, see Brass et al., 2005). Importantly, the results of our study expand these findings. Whereas in the work by Braver et al. (2003) participants had to shift between different tasks and select different task sets on a trial-by-trial basis, in the current study participants had to switch between different task orders and activate different task-order sets accordingly. Thus, by applying TMS in random-order DT blocks, we showed that the IFJ is not only recruited for implementing specific task information but also for selecting and activating task-order information that is specified by the task-order set.

The current results are also in line with data of Strobach et al. (2015). In this study, the authors intended to improve DT performance by applying transcranial direct current stimulation (tDCS) over the lPFC during fixed-order and random-order DT blocks as well as single-task blocks. Although stimulation did not affect performance in the single task, the authors showed that anodal tDCS over the left lPFC compared with sham stimulation resulted in improved performance (i.e., speeded RTs for both tasks) during DT blocks. An additional analysis revealed that this improvement was mainly evident in random-order compared with fixed-order blocks, which, according to the authors, provides preliminary evidence for a causal role of the lPFC in task-order coordination. However, several methodological characteristics of tDCS prevent a conclusive interpretation of these earlier findings. Compared with online TMS, tDCS is characterized by poor temporal as well as spatial resolution (Filmer, Dux, & Mattingley, 2014; Nitsche et al., 2008; Antal, Nitsche, & Paulus, 2006). In our study, we were able to circumvent these shortcomings because online TMS produces temporally highly focused effects (Sparing & Mottaghy, 2008) and can be used to interfere with neural information processing to a specific point in time on a trial-to-trial basis. Furthermore, TMS is characterized by a higher spatial resolution relative to tDCS (Priori, Hallett, & Rothwell, 2009); therefore, compared with Strobach et al. (2015), the current findings allow for a precise localization of brain regions with a causal role for task-order coordination.

Furthermore, we observed similar effects of IFJ TMS on performance in both same-order and different-order trials. This suggests that the IFJ's function in task-order coordination is not restricted to different-order trials, in which the task order changes relative to the previous trial (Szameitat et al., 2006), because in this case we should have observed selective effects of IFJ TMS in different-order trials compared with same-order trials. In recent studies (Kübler et al., 2018; Schubert, 2008; see also Hirsch, Nolden, & Koch, 2017), it was argued that performance differences between same-order and different-order trials reflect memory-based processes of task-order preparation. More specifically, task-order on a current trial is prepared in accordance with the task order in the previous trial. However, when the task order changes in different-order trials, the prepared task order has to be overcome, which results in additional processing demands compared with same-order trials. The data of the current study, however, indicate that the stimulation of the IFJ does not modulate these task-order coordination processes, which are specific for different-order trials. Instead, as we found equal effects of IFJ TMS on same-order and different-order trials, our results suggest that TMS of the IFJ interferes with task-order coordination processes during random-order blocks (i.e., for same-order and different-order trials alike), which are required for the active adjustment of the processing according to the sequence of stimuli.

It is important to note that, in addition to task-order coordination, the IFJ may implement further processes in DT situations. For example, evidence from DT studies using neuroimaging (Dux, Ivanoff, Asplund, & Marois, 2006) as well as noninvasive brain stimulation (Filmer, Mattingley, & Dux, 2013) suggests that the IFJ may also play an important role for executing response selection-related processes. Importantly, however, the aim of the current study was to investigate the causal relation between the IFJ and task-order coordination processes. For this purpose, TMS was applied during the CTI, that is, after the presentation of an instructional order cue and before the display of both target stimuli. The purpose of presenting this order cue was to temporally isolate task-order coordination processes from other processes that are crucial for performing the component tasks, such as perceptual or response selection-related processes (De Jong, 1995). Because of this temporal isolation, we were able to interfere with task-order coordination processes by applying TMS without impairing these other processes. Thus, the current findings do not contradict the findings of Dux et al. (2006) and Filmer et al. (2013). Instead they indicate that, in addition to response selection, the IFJ is also recruited for executing task-order coordination processes.

In contrast to the stimulation of the IFJ, TMS of the preSMA did not modulate task-order coordination processes in DT situations. This was indicated by the results of Experiment 2, according to which preSMA TMS did not affect DT performance, neither in trials from fixed-order blocks nor in trials from random-order blocks. This suggests that the preSMA does not play a causal role for task-order coordination. Nevertheless, the preSMA seems to be involved in implementing other processes relevant for DT processing as indicated by its increased BOLD response during DT situations (see Schubert & Szameitat, 2003; Szameitat et al., 2002). According to Soutschek et al. (2016), the preSMA contributes to resolving conflict between two tasks by inhibiting Task 2 processing, rather than implementing task-order coordination. This was shown by the fact that in that study preSMA TMS during the presentation of Task 2 results in faster RT2 compared with control conditions but leaves Task 1 performance undisturbed. The current findings do not contradict these findings of Soutschek et al. (2016). Instead, they extend these findings by indicating that the role of the preSMA is rather limited to resolving conflict between the two tasks and that it seems not to be involved in processes regulating task order in a DT situation. This is so because, in this study, we applied TMS during the CTI and not during Task 2 processing, as in the study of Soutschek et al. (2016), to exclusively interfere with the task-order coordination processes. As preSMA TMS did not affect DT performance, we conclude that the state of this brain region does not contribute to the implementation task-order coordination processes albeit it still may be causally involved in other processes required during DTs.

The results of our study add important insights to the field of DT research. A common finding from DT research is that RTs and error rates are usually increased in DT situations compared with situations in which only one of the two tasks is processed. A vast body of evidence indicates that these DT costs can be attributed to the serial processing at the response selection stage (Pashler, 1994)—although processing on perception and motor-related stages is usually carried out in parallel. This serial processing constitutes a bottleneck in the processing of temporally overlapping tasks. So far, DT research has mostly focused on questions considering basic attributes of this bottleneck (Koch, Poljac, Müller, & Kiesel, 2018). For example, on the one hand, a plethora of behavioral studies have tried to locate the central bottleneck within the stream of information processing or have investigated whether the bottleneck is structural (Pashler, 1994) or strategic (Logan & Gordon, 2001; Meyer & Kieras, 1997) in nature. On the other hand, multiple imaging studies aimed at pinpointing the bottleneck to certain brain structures (Spence, 2008; Dux et al., 2006; Marois & Ivanoff, 2005). More recent approaches in DT research examine how the cognitive system and the human brain deal with the additional requirements imposed by bottleneck processing. More specifically, as two tasks that have to be performed closely in time and compete for access to the bottleneck stage, the requirement for additional control processes arises that reduce resulting interference between the two temporally overlapping tasks (Schubert, 2008).

The findings of this study are consistent with views that active control processes, that is, task-order coordination processes, regulate the processing order of two tasks. These processes must be assumed in addition to a rather passive first-come, first-served principle suggested by classical response selection bottleneck models (De Jong, 1995; Pashler, 1994). Although the latter suggests a rather passive mechanism of deciding which task is processed first or second based on the arrival time of the tasks at the bottleneck (Strobach, Hendrich, Kübler, Müller, & Schubert, 2018), the former mechanism requires active monitoring and control processes. In line with recent accounts on task scheduling in DT situations (Szameitat et al., 2006; Luria & Meiran, 2003), these task-order coordination processes rely on order representations containing the sequence information of two task sets (instead of only one task representation). Importantly, by applying TMS we demonstrated that the lPFC is causally involved in task-order coordination in DT situations, supposedly by implementing the selection and activation of these higher order task-order representations. This adds to the findings from earlier studies assuming a similar function of the lPFC for maintaining and switching between representations of single tasks (Muhle-Karbe et al., 2014; Brass et al., 2005; Braver et al., 2003; Dove, Pollmann, Schubert, Wiggins, & Von Cramon, 2000). These order control processes complement other control mechanisms resolving conflict between two temporally overlapping tasks during DT processing on a more local level, for example, processes involved in the inhibitory control of one task stream during the ongoing processing of another task stream or resolving perceptual interference between the stimuli of the two tasks, and are associated with brain regions beyond the lPFC (Soutschek et al., 2016; Stelzel, Brandt, & Schubert, 2009; Jiang, 2004; Herath, Klingberg, Young, Amunts, & Roland, 2001). Taken together, these findings are in line with models assuming that interference processing in complex task situations such as DTs relies on multiple control mechanisms that are implemented by a number of different brain regions (Botvinick, Braver, Barch, Carter, & Cohen, 2001; Dosenbach, Fair, Cohen, Schlaggar, & Petersen, 2008). Operations executed by these brain regions allow for the monitoring and detection of information signaling potential interference as well as the appropriate and flexible adaptation of the cognitive system to different task demands. As indicated by our findings, among these brain regions, the lPFC seems to play a crucial role in regulating the sequence of two competing actions.

Conclusion

In this study, we investigated the causal role of the IFJ for task-order coordination by applying TMS in a DT with fixed and random order of the component tasks. We demonstrated that stimulation of the IFJ compared with control TMS conditions resulted in impaired DT performance in random order but not in fixed-order blocks. No such effect was found after preSMA TMS. These results indicate that the IFJ is causally involved in task-order coordination processes that are required to select and implement the appropriate processing order of two temporally overlapping tasks in DTs with variable task order.

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Ethics Approval

All procedures performed in studies involving human participants were in accordance with the ethics standards of the institutional and/or national research committee and with the 1964 Helsinki declaration and its later amendments or comparable ethical standards. Approval of the local ethics committee (Humboldt-Universität zu Berlin Department of Psychology) was obtained before the commencement of the study.

Note

1. Please note that a conjoined ANOVA combining both types of trials (same-order and different-order trials) provided evidence on a significance level with $p < .05$ that TMS over the IFJ affected task-order coordination processes in different-order trials to the same extent as in same-order trials . For that purpose, we analyzed RT1 using an ANOVA with the within-subject factors Trial Type (same-order trials, different-order trials) and TMS (no TMS, vertex TMS, IFJ TMS). In addition to the significant effect of the factor Trial Type, $F(2, 30) = 11.97$, $p < .01$, $\eta_p^2 = .44$, this analysis revealed a significant main effect of the factor TMS, $F(2, 30) = 8.52$, $p = .01$, $\eta_p^2 = .36$. Importantly, the interaction of the factors TMS and Trial Type did not reach significance, $F(2, 30) = 1.96$, $p = .16$, $\eta_p^2 = .12$, indicating a similar effect of IFJ TMS on same-order and different-order trials. These results are consistent with the assumption that stimulation of the IFJ results in impaired performance in random-order blocks irrespective of the specific trial type.

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ORIGINAL ARTICLE

The impact of free-order and sequential-order instructions on task-order regulation in dual tasks

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Abstract Dual tasks (DTs) are characterized by the requirement for additional mechanisms that coordinate the processing order of two temporally overlapping tasks. These mechanisms are indicated by two types of costs that occur when comparing DT blocks with fixed and random orders of the component tasks. On a block level, task-order control costs are reflected in increased reaction times (RTs) in random-order compared to fixed-order blocks, indicating global, monitoring-based, coordination mechanisms. On a trial level, within random-order blocks, order-switch costs are indicated by increased RTs on order switch compared to order repetition trials, reflecting memory-based mechanisms that guide task-order in DTs. To test the nature of these mechanisms in two experiments, participants performed DTs in fixed- and random-order blocks. In random-order blocks, participants were either instructed to respond to both tasks according to the order of task presentation (sequential-order instruction) or instructed to freely decide in which order to perform both tasks (free-order instruction). As a result of both experiments, we demonstrated that task-order control costs were reduced under the free-order compared to the sequential-order instruction, whereas order-switch costs were not affected by our instruction manipulation. This pattern of results suggests that the task-order control costs reflect global processes of task-order regulation such as engaging monitoring processes that are sensitive to changes in order instructions, while order-switch costs reflect rather local memory-based mechanisms that occur irrespective of any effort to coordinate task-order.

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Introduction

Human performance is usually impaired in situations in which multiple tasks are performed simultaneously compared to situations in which the same tasks are performed separately. This can be shown in the dual-task (DT) paradigm, in which two choice reaction time tasks are performed simultaneously. In this paradigm, DT costs occur, which are reflected in slower reaction times (RTs) and/or increased error rates relative to single-task situations. DT costs are often explained by the assumption of a central capacity limitation (i.e., a bottleneck) at the response selection stage that requires the serial processing of both tasks (Pashler, 1994; Schubert, 1999, 2008). In previous years, research has addressed various questions regarding this bottleneck, for example, whether it is structural (Pashler, 1994) or strategic (Meyer & Kieras, 1997) in nature. However, irrespective of this and similar debates, until now, it still remains unknown how the processing order of two tasks is regulated at the central bottleneck stage. The aim of the current study is to investigate the mechanisms enabling humans to schedule the processing of two temporally overlapping tasks, and how these mechanisms are affected by different environmental demands such as task instructions.

Classical bottleneck models (Pashler, 1994) assume a rather passive scheduling mechanism that allocates the bottleneck to both tasks according to their arrival time at the bottleneck stage. However, many studies suggest that bottleneck processing does not necessarily result from a passive first-come-first-served scheduling. Instead, DT situations require additional mechanisms that regulate and guide the processing order of two tasks that compete for access to a capacity-limited or serially operating bottleneck (DeJong, 1995; Logan & Gordon, 2001; Luria & Meiran, 2003, 2006; Schubert, 1999, 2008; Sigman & Dehaene, 2006; Szameitat, Schubert, Müller, & von Cramon, 2002; Szameitat, Lepsien, von Cramon, Sterr, & Schubert, 2006).

Evidence for these mechanisms in DT situations comes from studies comparing DT performance in blocks with constant order and blocks with random order of both tasks (DeJong, 1995; Strobach, Soutschek, Antonenko, Flöel, & Schubert, 2015; Stelzel, Kraft, Brandt, & Schubert, 2008; Szameitat et al., 2002): in the study of Szameitat and colleagues, participants performed a DT consisting of an auditory and a visual choice reaction time task. DT trials were presented in two types of blocks: in fixed-order blocks, both stimuli were presented with a constant order throughout the whole block, i.e., either the visual stimulus as the first stimulus, or the auditory stimulus as the first stimulus. In random-order blocks, on the contrary, the presentation order of both stimuli varied randomly from trial to trial and unbeknownst to participants. Most importantly, participants were instructed to respond to both stimuli according to the order of their presentation. When comparing DT performance in these two kinds of blocks, task-order control costs arise, which are indicated by increased RTs and error rates for the tasks in random-order compared to fixed-order blocks. According to the authors, the increase in RTs reflects additional control processes that are required to coordinate the processing order of both tasks in random-order blocks but that are not required (or required to a lesser degree) in fixed-order blocks. Further evidence for these control processes comes from data of functional magnetic resonance imaging. These data revealed increased activation during random-order compared to fixed-order blocks in the lateral prefrontal cortex, a brain region reliably shown to be involved in cognitive control processes (Szameitat et al., 2002; see also Stelzel et al., 2008).

Moreover, evidence for mechanisms that guide the processing order in DTs comes from a similar line of research that investigated these mechanisms on a more fine-grained trial-by-trial level (DeJong, 1995; Luria & Meiran, 2003, 2006; Szameitat et al., 2006). In the study of Szameitat et al. (2006), participants performed a DT consisting of an auditory and a visual task in random-order blocks. Within these random-order blocks, the authors distinguished between two trial types: on same-order trials, response order was the same compared to the previous trial, e.g., on both trials, the visual task was responded to first and the auditory task second. On different-order trials, on the contrary, response order was reversed relative to the preceding trial, e.g., on the previous trial, the visual task was responded to first and the auditory task second, but on the next trial, the auditory task was responded to first and the visual task second. When comparing DT performance on both trial types, order-switch costs arise, which are indicated by slower RTs in different-order compared to same-order trials. In addition to blockwise task-order control costs, the occurrence of these trialwise costs provides sufficient evidence for control mechanisms that regulate and guide the processing order in DTs.

Although both task-order control and order-switch costs have been shown to be reliable phenomena (see also Stelzel et al., 2008; Strobach et al., 2015; Szameitat et al., 2002, 2006), the specific mechanisms underlying these two types of costs are still a matter of debate. Several studies have shown that performance parameters differ between task-order control costs and order-switch costs (Luria & Meiran, 2003) and that non-invasive stimulation protocols have differential impacts on these costs (Strobach et al., 2015). Hence, it is tempting to assume that both cost types reflect distinct mechanisms regulating and guiding the processing order of two tasks in DTs.

In more detail, recent studies proposed that order-switch costs may reflect memory-based mechanisms of task-order regulation. According to Schubert (2008; see also Hirsch, Nolden & Koch, 2017), the processing order of two tasks on a current trial can be prepared in advance based on the processing order on the previous trial: after the execution of a DT trial, information about task order is stored in episodic memory. This episodic order trace remains active over time and influences the DT performance on the subsequent trial. On same-order trials, this results in a performance benefit as the order trace primes the processing order of the previous trial. This is similar to single-task situations, in which automatic priming between repeating stimuli and/or responses in sequential task trials has been shown to have tremendous effects on response times in a number of studies (Hommel, 2004). On different-order trials, the activation of the order trace has to be overcome and the alternative processing order has to be initiated to switch the processing order, which causes additional processing costs. Thus, order-switch costs seem to reflect priming-related and transient memory-based mechanisms of task-order guidance that arise on a trial-by-trial level.

Unlike order-switch cost, task-order control costs seem to reflect rather global monitoring-based mechanisms of task-order regulation (Stelzel et al., 2008; Strobach et al., 2015). In fixed-order blocks, in which the two component tasks are presented with constant stimulus order, the demands on such monitoring-based mechanisms are reduced as participants can use the same task scheduling strategy throughout the entire block. In random-order blocks, however, the order of stimuli varies permanently. Since participants are asked to respond to the stimuli in the order of occurrence, they have to monitor the order of stimuli and permanently adjust the task processing order; this results in additional task-order control costs in random-order compared to fixed-order blocks. Preliminary evidence for the assumption that these costs reflect monitoring-based processes comes from a study showing that task instructions modulate DT performance (DeJong, 1995; see also Hendrich, Strobach, Müller, & Schubert, 2017 submitted). In this study, DeJong (1995, Experiment 2) presented DT trials in random-order blocks and tested two groups with different task instructions requiring different degrees of task-order monitoring: one group received a sequential-order instruction requiring participants to respond to both stimuli according to the order of their presentation. Thus, this group had to monitor and to adjust the processing order to a normative (pre-instructed) task-order specified by the stimulus sequence. The other group received a free-order instruction and could freely decide which task to perform first and which task second. The results showed that RTs for both tasks were faster in the free- compared to the sequential-order instruction group. Further, DeJong also analyzed the number of task-order reversal trials. In these trials, participants respond to the tasks in a reversed order relative to the order of stimulus presentation, e.g. if the visual stimulus is presented first and the auditory stimulus second, the response for the auditory task is given first and the response for the visual task second. When comparing both groups, the sequential-order instruction group produced less task-order reversals than the free-order instruction group.

According to DeJong (1995), these results indicate increased demands on global monitoring-based mechanisms of task-order regulation in the sequential-order group. The participants of this group have to monitor the sequence of stimuli, decide about the appropriate task-order corresponding to the perceived stimulus sequence, and adjust their processing order accordingly. This adjustment and the corresponding decrease in task-order reversals come, however, at the cost of increased RTs. In the free-order instruction group, in contrast, performance can be accomplished with less reliance to the stimulus order and with more reliance on an internally chosen order, which results in decreased RTs and increased task-order reversal rates relative to the sequential-order group.

The findings by DeJong (1995) give first evidence for the fact that instructions modulate task-order regulation processes in general. However, due to methodological issues, it is hard to draw clear conclusions about the specific effects of instructions on task-order control and order-switch costs, as well as their underlying mechanisms. First, DeJong only assessed DT performance on random-order blocks and did not include fixed-order blocks in his design. The latter would have been necessary to test the effect of instructions on task-order control costs, i.e., RT differences between fixed- and random-order blocks. Second, within the random-order blocks, the author did not distinguish between same-order and different-order trials, which makes it impossible to evaluate the impact of instructions on order-switch costs. The aim of this study was to disentangle the effect of instructions on task-order control and order-switch costs and their underlying mechanisms.

Rationale of the study

To dissociate the effects of instructions on monitoring- and memory-based mechanisms that are employed during DTs, we administered an instruction manipulation similar to the one used by DeJong (1995) and applied the following design logic (for a similar approach for task switching, see Rubin & Meiran, 2005): if an instruction manipulation affects monitoring-based mechanisms of task-order regulation, it should influence performance in random-order blocks compared to fixed-order blocks, i.e., the task-order control costs. In contrast, if the same manipulation has an impact on memory-based mechanisms of task-order guidance, the instruction would affect order-switch costs, i.e., the RT difference between same- and different-order trials.

In two experiments, participants performed a DT in fixed- and random-order blocks. In random-order blocks, participants received either a free-order or a sequential-order instruction. Under the free-order instruction, participants could respond to both tasks in the order they preferred, which should reduce the demands on monitoring-based mechanisms as there was neither a need to keep track of the stimulus order nor to match the processing order (like under the sequential-order instruction). We expected task-order reversal rates to increase under the free-order compared to the sequential-order instruction, as participants could base the processing order of both tasks on their free-order choice and not according to the normative stimulus order. In addition, we expected task-order control costs to decrease under the free-order compared to the sequential-order instruction due to decreased demands on monitoring-based mechanisms. Note that this hypothesis is in line with evidence from research on voluntary task switching, which showed faster RTs for situations with free task choice compared to situations with cued task choice (e.g., Mayr & Bell, 2006; but see Arrington & Logan, 2005).

Unlike task-order control costs, order-switch costs reflect mechanisms that guide the processing order by pre-activating an episodic memory trace containing the processing order of the tasks in the previous trial. These mechanisms should occur irrespective of any active effort to regulate the processing order according to instructions if they are based on a rather automatic activation of the processing order's memory trace from the previous trial. Several authors (Hommel, 2004; Mayr, 2002) showed that the repetition of certain task components between sequential trials can influence task performance independently of the operation of effortful control processes. Consequently, order-switch costs based on automatic pre-activation of the task order from a previous trial should be unaffected by an instruction manipulation requiring the adjustment of performance according to a normative task-order. Alternatively, it could be that in DT situations, task instructions affect priming-based mechanisms of response-order regulation, because some studies have shown that top–down control can interact with trial-based priming effects in task-switching situations (Dreisbach & Haider, 2006; Koch & Allport, 2006).

Experiment 1

In Experiment 1, the goal was to disentangle the effect of order instructions on monitoring- and memory-based mechanisms regulating and guiding task order in DTs. For that purpose, two groups of participants performed a DT in fixed- and random-order blocks under two different task-order instructions. One group was instructed to respond to both tasks according to stimulus order (sequential-order instruction) and the other group was instructed to freely decide about their response order (free-order instruction). We compared task-order control and order-switch costs under both instructions and hypothesized that task-order control cost should be reduced under the free-order compared to the sequential-order instruction.

Materials and methods

Participants

Fifty participants (40 female) took part in the experiment. Participants were randomly allocated to one of the two instruction groups. Half of the participants received the sequential-order and the remaining half the free-order instruction in random-order blocks. Mean age was 25.07 years (SD = 4.22 years). All participants reported normal or corrected-to-normal vision and hearing and received either course credit or payment (8 Euros/h) for their participation. Informed consent was obtained from all participants.

Apparatus and stimuli

The experiment was programmed in Presentation (Version 18.0 12.05.14) and run on a Dell Optiplex 760. Visual stimuli were presented on a 24 inch LCD monitor at a resolution of 1920 × 1080 pixels with a refresh rate of 144 Hz at a viewing distance of 80 cm. For the visual task, one of three white colored squares differing in size was presented centrally on a black background for 200 ms: a small square (1.8° × 1.8°), a medium-sized square (2.36° × 2.36°), or a large square (3.54° × 3.54°). Participants responded by pressing the ",", ".", and "-"-key on a QWERTZ keyboard with their right index, middle, and ring finger, respectively. Auditory stimuli were presented for 200 ms and consisted of three sine-wave tones with frequencies of 200, 650, and 1100 Hz. Participants were instructed to respond by pressing the "y", "x", and "c"-key with their left ring, middle, and index finger, respectively.

Design and procedure

The trial structure is shown in Fig. 1. Each trial began with the presentation of a fixation cross for 750 ms that was followed by a blank screen for 250 ms. Subsequently, both stimuli were presented sequentially for 200 ms each and separated by a constant SOA of 200 ms. After presentation of the stimuli, the screen was cleared for a response period of maximum 2850 ms, which was followed by an intertrial interval (ITI) of 750 ms. Error feedback was given for omitted responses as well as incorrect stimulus discrimination and consisted of the German words 'ZU LANGSAM' (too slow) or 'FALSCH' (incorrect), respectively. The feedback was presented centrally for 500 ms during the ITI.

DT trials were presented in fixed-order and random-order blocks. In fixed-order blocks, the order of stimulus presentation remained constant throughout the whole block (either blocks with the visual stimulus first or blocks with the auditory stimulus first). In random-order blocks, the stimulus order varied randomly from trial to trial with the visual task occurring first in half of all trials. In addition, we controlled for the occurrence of 50% trials with repetitions and switches of stimulus order relative to the previous trial.

Task instruction was manipulated on a group level: in random-order blocks, half of the participants received the free-order instruction, and the other half received the sequential-order instruction. In the sequential-order group,

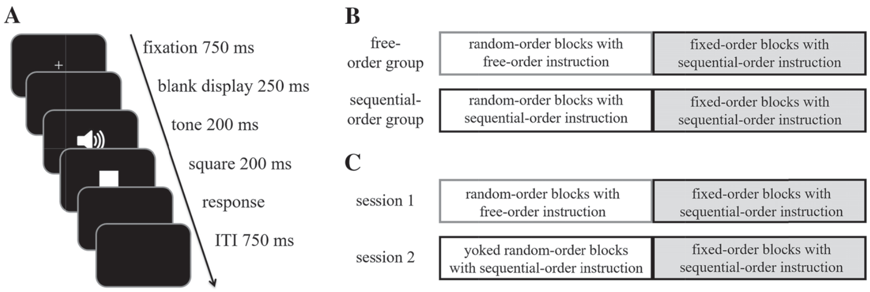

Fig. 1 Trial and block design for both experiments. **a** Time course for an exemplary DT trial in which the tone was presented first is shown on the *left*. **b** Block sequence for Experiment 1: on random-order blocks, the free-order group and the sequential-order group received the free-order and sequential-order instructions, respectively. After finishing the random-order blocks, participants of both groups performed fixed-order blocks with the sequential-order instruction. **c** Block and session sequence for Experiment 2: on session 1, participants first performed random-order DT blocks with a free-order instruction that were followed by fixed-order blocks with the sequential-order instruction. On session 2, participants first performed yoked random-order DT blocks with a sequential-order instruction, which were followed by fixed-order blocks with the same instruction. *ITI* intertrial interval, *white* and *light grey boxes* indicate random-order and fixed-order blocks, respectively, *grey* and *black frames* indicate blocks with the free- and the sequential-order instruction, respectively

participants were instructed to respond "on each trial as fast and accurately as possible to both stimuli in the same order in which they were presented". In the free-order group, participants were instructed to respond "on each trial as fast and accurately as possible to both stimuli and to freely decide which task to perform first". The free-order group was additionally instructed not to use a systematic response pattern, e.g., always reacting to the same task first or constantly alternating response orders between trials (DeJong, 1995). Note that this additional requirement might have increased processing demands during random-order blocks with the free-order instruction as participants have to exert top–down control to prevent systematic biases in their order choice. However, this additional instruction was necessary, as it prevents the most likely strategy, namely, to stay with a fixed response order, and, thus, guarantees a comparable amount of same- and different-order trials (for a similar approach in voluntary task switching, see Arrington & Logan, 2005).

In fixed-order blocks, all participants received the sequential-order instruction. This was necessary, as task-order control costs reflect additional processes that are required in DT blocks with variable task-order compared to DT blocks with fixed task-order. Thus, applying the sequential-order instruction in fixed-order blocks for both groups guaranteed a constant task-order in these blocks and allowed investigating whether additional processing demands in random-order compared to fixed-order blocks are modulated by different instructions.

At the beginning, participants completed four practice blocks: two single-task blocks with 12 trials and two random-order DT blocks with 18 trials each. The main experiment consisted of two parts: in the first part, participants performed six random-order blocks consisting of 72 trials each. These trials resulted from all possible combinations of visual stimuli (small, medium, and large square), auditory stimuli (200, 650, and 1100 Hz), order of stimuli on the present (auditory stimulus first and visual stimulus first), and the previous trial (repetition of stimulus order and switch of stimulus order). In the second part, after random-order blocks, DTs were presented in four fixed-order blocks under the sequential-order instruction for both groups with 72 trials each. In half of the fixed-order blocks, the auditory stimulus was presented first; in the other half, the visual stimulus was presented first. Random-order blocks were always administered before fixed-order blocks to avoid biasing participants' order choices in random-order blocks under the free-order instruction based on a previous fixed response order and its instruction in fixed-order blocks.

Results

Participants' RTs for the first task (task 1, RT1) and the second task (task 2, RT2) and task-order reversals, i.e., trials on which participants gave their responses in a reversed order relative to the order of stimuli, were used as dependent variables. For RT analyses, trials with RTs longer or shorter than ± 2.0 standard deviations for each participant and condition as well as trials with incorrect or omitted responses were excluded ($m = 11.69\%$). In addition, for fixed- and random-order blocks with the sequential-order instruction, trials with task-order reversals ($m = 8.05\%$) were excluded from RT analyses, as participants were instructed to match their response order to the order of stimuli. We investigated RTs with two main analyses. First, to analyze task-order control costs, RTs from fixed- and random-order blocks were compared between both groups. In a second analysis, the effect of the instruction manipulation on order-switch costs was investigated by analyzing RTs from same- and different-order trials. Mean RTs for block and trial types were pooled across trials with the auditory and the visual stimulus presented first. Analyses of variances (ANOVAs) and post-hoc *t* tests were calculated using a significance threshold of 5%.

Task-order reversals

Under the free-order instruction, participants responded to the auditory stimulus first on 50.29% of the trials in random-order blocks, which indicates no strategic preference for one of the two potential response orders. Task-order reversals were analyzed to test whether participants followed the given instruction. According to DeJong (1995), larger amounts of task-order reversals should occur in the free-order compared to the sequential-order instruction group in random-order blocks. The percentages of task-order reversals are illustrated in Fig. 2 and were analyzed using an ANOVA with the within-subjects factor block type (fixed-order block, random-order block) and the between-subjects factor instruction group (sequential-order group and free-order group). This analysis revealed a main effect of the factor block type, $F(1, 48) = 137.39$, $p < .001$, $\eta^2 = .74$, showing that participants committed more task-order reversals in random-order [mean (m) = 20.06%] than in fixed-order blocks (m = 3.30%). Furthermore, the free-order group produced more task-order reversals (m = 20.22%) than the sequential-order group (m = 6.55%), $F(1,48) = 41.37$, $p < .001$, $\eta^2 = .46$. Most importantly, we found a significant block type × instruction group interaction, $F(1,48) = 19.37$, $p < .001$, $\eta^2 = .29$. Subsequent pairwise comparisons revealed that the increase in task-order reversals from fixed-order to random-order blocks was much larger in the free-order group (m = 28.35%) compared to the sequential-order group (m = 12.87%), $t(48) = 4.40$, $p < .001$. This pattern of results indicates that, in random-order blocks, the free-order group, in accordance with their instruction, performed both tasks with less reliance to the order of stimuli compared to the sequential-order group.

Note, however, that the participants' processing order under the free-order instruction was still biased in a bottom–up way by the order of stimuli. Though task-order reversal rates in random-order blocks were higher in the free-order (m = 34.84%) compared to the sequential-order group (m = 12.98%), $t(48) = 7.64$, $p < .001$, this percentage differed from a task-order reversal rate of 50% that one would expect in the free-order group if participants based their order choice solely on a "free" decision, $t(24) = 8.92$, $p < .001$. Thus, under the free-order instruction, the processing order of both tasks was not only influenced top–down by participants' free order choices, but also bottom–up by the order of stimuli on a given trial.

Task-order control costs

To test whether task-order control costs were reduced in the free-order group, we performed an ANOVA on RTs with the within-subjects factor tasks (task 1 and task 2) and block type (fixed-order and random-order blocks) and the between-subjects factor instruction group (sequential-order group and free-order group). This analysis revealed a significant main effect of the factor task, $F(1, 48) = 37.19$, $p < .001$, $\eta_p^2 = .44$. As can be seen in Table 1, RTs for task 1 (m = 994 ms) were faster than those for task 2 (m = 1082 ms). The main effect of the factor block type was also significant, $F(1, 48) = 190.87$, $p < .001$, $\eta_p^2 = .80$, indicating task-order control costs, i.e., slowed responses in random-order (m = 1151 ms) compared to fixed-order blocks (m = 926 ms).

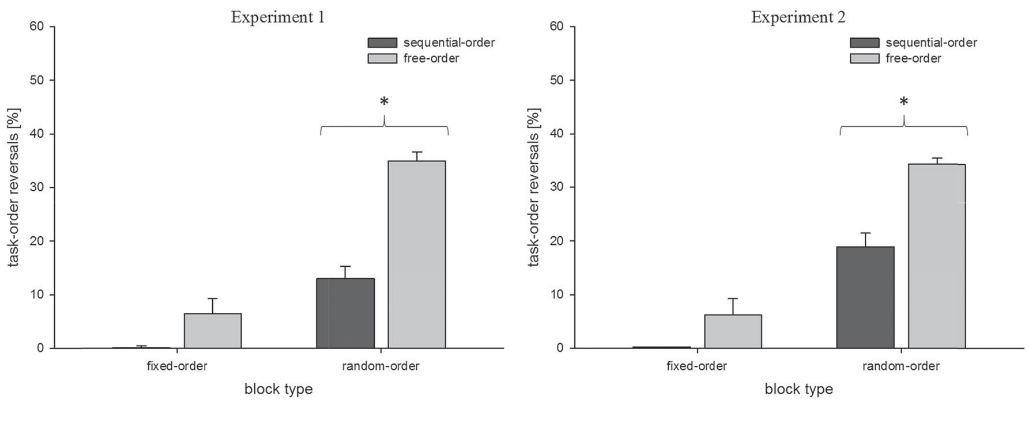

Fig. 2 Task-order reversals in % from Experiment 1 (*left panel*) and Experiment 2 (*right panel*) as a function of the factor block type (fixed-order blocks and random-order blocks), and instruction (sequential-order instruction and free-order instruction). *Error bars* denote the standard error of the mean. *Asterisks* denote a significant difference in task-order reversals between both instruction groups in random-order blocks ($p < 0.01$)

Table 1 Mean reaction times (and standard error of the mean) (in milliseconds) from Experiment 1 and Experiment 2 for Task 1 (RT1) and Task 2 (RT2) for each block (left) and trial type (right) depending on the instruction condition

Group	Experiment 1							
	Block type				Trial type from random-order blocks			
	Fixed-order blocks (with sequential-order instruction)		Random-order blocks (with group specific instruction)		Same-order trials (with group specific instruction)		Different-order trials (with group specific instruction)	
	RT1	RT2	RT1	RT2	RT1	RT2	RT1	RT2
Free-order	909 (209)	988 (242)	1056 (196)	1153 (238)	1021 (191)	1114 (235)	1092 (207)	1191 (246)
Sequential-order group	869 (257)	938 (252)	1143 (281)	1250. (282)	1110 (277)	1219 (275)	1176 (289)	1282 (295)

Session	Experiment 2							
	Block type				Trial type from random-order blocks			
	Fixed-order blocks (with sequential-order instruction)		Random-order blocks (with session specific instruction)		Same-order trials (with session specific instruction)		Different-order trials (with session specific instruction)	
	RT1	RT2	RT1	RT2	RT1	RT2	RT1	RT2
Session 1	939 (205)	1038 (252)	1119 (190)	1255 (254)	1081 (179)	1210 (255)	1156 (190)	1300 (260)
Session 2	909 (190)	990 (242)	1208 (233)	1319 (267)	1165 (236)	1272 (275)	1251 (238)	1366 (267)

The main effect of instruction group was not significant, $F(1, 48) < 1$, $p = .97$, $\eta_p^2 < .001$. However, we found a significant block type × instruction group interaction, $F(1, 48) = 17.82$, $p < .001$, $\eta_p^2 < .27$, suggesting that the factor instruction group modulated RT differences between fixed- and random-order blocks. Subsequent comparisons revealed that task-order control costs, i.e., the RT increase from fixed- to random-order blocks, were larger in the sequential-order ($m = 294$ ms) compared to the free-order group ($m = 156$ ms), $t(48) = 4.22$, $p < .001$ (see Fig. 3). This finding is in line with the assumption that the instruction manipulation affects monitoring-based mechanisms of task-order regulation.

In the following, we analyzed in more detail what the observed instruction-based influence on task-order control costs means for the specific response times of the two instruction groups in the fixed- and random-order blocks. According to DeJong (1995), one would expect that the free-order group should show faster RTs in random-order blocks compared to the sequential-order group, because in these blocks, the free-order group can perform DTs with less reliance on monitoring-based task-order regulation processes compared to the sequential-order group. In fixed-order blocks, on the contrary, groups should show similar DT performance as they both received the sequential-order instruction. As can be seen in Table 1, RTs on fixed-order blocks did not differ between the free- ($m = 948$ ms) and the sequential-order group ($m = 903$ ms), $t(48) = .68$, $p = .50$. On random-order blocks, on the contrary, the free-

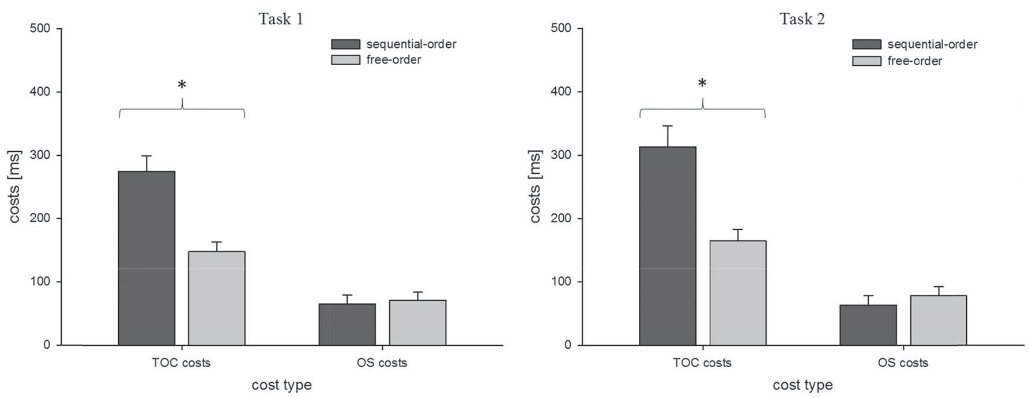

Fig. 3 Task-order control (TOC) costs and order-switch (OS) costs from Experiment 1 as a function of the factor instruction (sequential-order instruction and free-order instructions). *Error bars* denote the standard error of the mean. *Asterisks* denote a significant difference in task-order control costs between both instruction groups ($p < 0.01$). *Left panel* costs for task 1. *Right panel* costs for task 2

order group showed faster RTs ($m = 1105$ ms) relative to the sequential-order group ($m = 1197$ ms). However, this difference was not significant, $t(48) = 1.33$, $p = .19$. Note that despite the non-significant difference in RTs between both groups in random-order blocks, we found a decrease of task-order control costs in the free-order compared to the sequential-order group.

Furthermore, we found a significant task × block-type interaction, $F(1, 48) = 9.26$, $p = .004$, $\eta_p^2 = .16$, reflecting a reduced RT increase from task 1 to task 2 in fixed- ($m = 74$ ms) relative to random-order blocks ($m = 102$ ms), $t(48) = 3.03$, $p = .004$. In our view, this result suggests that if task-order is predictable under the condition of fixed-order blocks compared to random-order blocks, participants can also prepare for the switch from task 1 to task 2 (DeJong, 1995; Liepelt, Strobach, Frensch, & Schubert, 2011). Such a prepared switch allows for the reduced increase of RTs from task 1 to task 2 in fixed-order compared to random-order block for the two instruction conditions. Other interactions were not significant (all $ps > .24$).

Order-switch costs

To test whether order-switch costs were affected by instructions, we performed an ANOVA on RTs with the within-subjects factor tasks (task 1 and task 2), trial type (same-order trial and different-order trial), and the between-subjects factor instruction group (sequential-order group and free-order group). This analysis revealed a significant main effect of the factor task, $F(1, 48) = 39.18$, $p < .001$, $\eta_p^2 = .45$. As can be seen in Table 1, RTs for task 1 ($m = 1100$ ms) were faster than RTs for task 2 ($m = 1201$ ms). In addition, RTs increased from same-order ($m = 1116$ ms) to different-order trials ($m = 1185$ ms), $F(1, 48) = 49.55$, $p < .001$, $\eta_p^2 = .51$, indicating the occurrence of order-switch costs. There was no difference in RTs between both instruction groups, $F(1, 48) = 1{,}77$ $p = .19$, $\eta_p^2 = .04$. Importantly, the interaction of trial type and instruction was not significant, $F(1, 48) = .25$, $p = .62$, $\eta_p^2 < .01$, indicating that order-switch costs did not differ between instruction conditions[1]. No other interactions were significant (all $ps = .24$).

[1] To further analyze the lacking effect of the instruction manipulation on order switch costs, we applied Bayesian-like interference testing. According to Wagenmakers (2007), we tested the posterior probability ($Pr(H_0|D)$) of a hypothesis assuming a missing interaction of the factors Instruction group and Trial type versus a hypothesis assuming a significant interaction of these factors by calculating the Bayesian information criterion (BIC) between both. With $\Delta BIC = 3.66$ and $Pr(H0|D) = 0.86$ this analysis provides 'positive' evidence for the assumption that order-switch costs did not differ between both groups.

Discussion

The main finding of Experiment 1 is that (1) task-order control costs, i.e., RT differences between fixed- and random-order blocks, were reduced in the free-order compared to the sequential-order group and (2) order-switch costs, the RT difference between same- and different-order trials, were unaffected by the instruction manipulation. This is in line with the assumption that task-order control costs reflect monitoring-based processes of task-order regulation that are less employed under the free-order instruction. Order-switch costs, on the other hand, seem to reflect memory-based mechanisms of task-order guidance that are unaffected by the particular instruction manipulation applied in this study.

However, two puzzling findings of the current experiment need to be discussed and explored in a further experiment. First, although we found a modulation of task-order control costs by instructions, we could not replicate the finding of DeJong (1995) that RTs from random-order blocks differed between the two instruction groups. One reason for this result might be differences between the applied designs: in contrast to DeJong, we applied a fixed sequence of blocks (first random-order than fixed-order blocks). This fixed block sequence might have confounded our results. For example, different instructions on random-order blocks might have distinctively modulated performance on subsequent fixed-order blocks and, thus, led to differences in task-order control costs. Note that such carry-over effects would result in performance differences in fixed-order blocks between the two instruction groups. As reported, however, RTs on fixed-order blocks did not differ between both groups making the occurrence of instruction-dependent carry-over effects on fixed-order blocks rather unlikely. Alternatively, while in the present study, participants were tested on one single session and the different instructions were varied on a group level, DeJong manipulated his instruction on a within-subject level and tested his participants on three consecutive sessions. These differences in the study of DeJong may have, in contrast to the present study, facilitated observing RT differences between both instruction conditions on random-order blocks.

Second, a potential caveat of Experiment 1 is related to differences in the frequency of response order switches occurring during random-order blocks between the two groups. Research from task switching has shown that the frequency of task switches is usually reduced in voluntary task switching compared to situation with pre-defined task switches (e.g., Arrington & Logan, 2005; Reuss, Kiesel, Kunde, & Hommel, 2011). The reason for this is that when participants are instructed to freely choose between tasks, they usually tend to repeat tasks more often as this exposes less processing effort compared to frequent task switches. Similarly, in Experiment 1, during random-order blocks, participants from the free-order group showed a similar tendency to a reduced number of response-order switches ($m = 32.41\%$) relative to the sequential-order group ($m = 41.07\%$), $\chi^2(1) = 6.61$, $p = .01$. This difference in the order-switch frequency might have confounded our results. For example, it is conceivable that the overall task difficulty in random-order blocks increases with an increasing number of response-order switches. This may explain the observation of increased RTs on random-order compared to fixed-order blocks, which were especially prevalent in the sequential-order compared to the free-order group. Thus, an unequal number of order switches between groups might have resulted in increased task-order control costs in the sequential-order compared to the free-order group. In Experiment 2, we controlled for possible confounding influences of different-order-switch frequencies across conditions by applying a yoked design.

Experiment 2

The goal of Experiment 2 was to investigate the effect of instructions on monitoring-based processes of task-order regulation and to control for possible confounding effects that might have been related to different response order switch rates across the conditions in Experiment 1. For that purpose, we administered DT trials for both instruction conditions in a yoked design and varied task instructions as a within-subjects manipulation. To do so, we first administered random-order blocks with the free-order instruction, which provided us with a sequence of chosen task orders across the experimental condition individually for each participant. Subsequently, participants performed again a condition with random-order blocks but now with the sequential-order instruction; most importantly, in this sequential-order instruction condition, we presented the stimulus order for the two tasks on each DT trial in yoked fashion with the participants' chosen order in the initial free-order instruction condition (for a similar approach in task-switching, see also Masson & Carruthers, 2014). This yoking procedure should ensure similar order-switch rates in random-order blocks for both instruction conditions and it allowed us to apply a within-subjects manipulation of task-order instruction as was the case in DeJong (1995). As in Experiment 1, we hypothesized that task-order control costs should be reduced under the free-order compared to the sequential-order instruction.

Materials and methods

Participants

Twenty-five participants (23 female) took part in the experiment. Mean age was 22.16 years (SD = 2.69 years). All participants reported normal or corrected-to-normal vision and hearing and received either course credit or payment (8 Euros/h) for their participation. Informed consent was obtained from all participants included in the study. One participant did not return for the second session and her data were excluded from analyses.

Apparatus and stimuli

Apparatus and Stimuli were the same as in Experiment 1.

Design and procedure

Similar to Experiment 1, participants performed DTs in fixed- and random-order blocks. Differently to Experiment 1, the instruction manipulation was applied as a within-subjects factor. For this purpose, participants were tested on two sessions: on the first session (session 1), participants received the free-order instruction in random-order blocks, and on the second session (session 2), they received the sequential-order instruction. This sequence of the instruction conditions was chosen for two reasons. First, it guaranteed that participants' order choice in the free-order instruction condition was not affected by any prior experience with the sequential-order instruction. Second, in random-order blocks of session 2, stimulus order did not vary randomly, but instead, it was yoked with the individually selected response order of each participant in the session 1. The aim of this yoked design was to guarantee comparable order-switch rates across instruction conditions. On both sessions, after performing four random-order blocks, participant performed two fixed-order blocks with the sequential-order instruction (see Experiment 1). Blocks consisted of 72 trials each. Both sessions were separated by 7–10 days.

Results

The analytic procedure was similar to that of Experiment 1, except that session was included as a within-subjects factor in the analyses. Trials with RTs longer or shorter than ± 2.0 standard deviations for each participant and condition as well as trials with incorrect or omitted responses ($m = 12.81\%$) were excluded from RT analyses, as well as task-order reversals ($m = 8.43\%$) from blocks with the sequential-order instruction.

Task-order reversals

In random-order blocks under the free-order instruction, participants responded on 51.60% of trials to the auditory stimulus first, which indicated no preference for one of the two potential response orders. The percentages of task-order reversals are illustrated in Fig. 2 and were analyzed

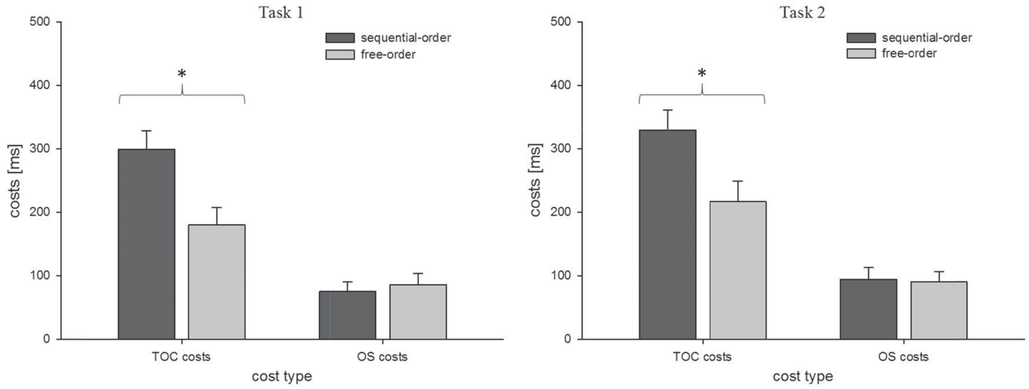

Fig. 4 Task-order control (TOC) costs and order-switch (OS) costs from Experiment 2 as a function of the factor Instruction (sequential-order instruction and free-order instructions). *Error bars* denote the standard error of the mean. *Asterisks* denote a significant difference in task-order control costs between both instruction groups ($p < 0.01$). *Left panel* costs for task 1. *Right panel* costs for task 2

by applying an ANOVA with the within-subjects factor block type (fixed-order block and random-order block) and session (session 1 with free-order instruction and session 2 with sequential-order instruction). This analysis revealed a main effect of the factor block type, $F(1, 23) = 148.70$, $p < .001$, $\eta_p^2 = .87$, suggesting that participants committed more task-order reversals in random-order blocks ($m = 26.59\%$) than in fixed-order blocks ($m = 3.18\%$). Furthermore, participants produced more task-order reversals in session 1 with the free-order instruction ($m = 22.22\%$) than on the session 2 with the sequential-order instruction ($m = 9.55\%$), $F(1,23) = 39.13$, $p < .001$, $\eta_p^2 = .63$. In addition, we also found a significant block type × session interaction, $F(1,40) = 9.36$, $p = .01$, $\eta_p^2 = .32$, revealing that the effect of block type differed between sessions. Pairwise comparisons revealed that the increase in task-order reversals from fixed- to random-order blocks was larger on session 1 with the free-order instruction ($m = 28.05\%$) compared to the session 2 with sequential-order instruction ($m = 18.75\%$), $t(23) = 2.18$, $p < .04$. Thus, on session 1, when receiving the free-order instruction on random-order blocks, participants performed both tasks with less reliance to the stimulus order compared to session 2 with the sequential-order instruction.

Under the free-order instruction, order reversal rates on random-order blocks ($m = 34.24\%$) differed significantly from a task-order reversal rate of 50% that one would expect if participants' processing order did only rely on their free order choice, $t(23) = 14.06$, $p < .\ 001$. In line with the similar observation in Experiment 1, this pattern suggests that, in addition to their order choices, also the actual order of stimuli on a given trial influences participants' response order on random-order blocks with a free-order instruction. Nevertheless, task-order reversal rates on random-order blocks were still higher under the free-order instruction compared to the sequential-order instruction ($m = 18.93\%$), $t(23) = 6.30$, $p < .001$.

Task-order control costs

To test whether task-order control costs were reduced under the free-order instruction, we performed an ANOVA on RTs with the within-subjects factor tasks (task 1 and task 2), block type (fixed-order and random-order blocks), and session (session 1 with free-order instruction and session 2 with sequential-order instruction). This analysis revealed a significant main effect of the factor task, $F(1, 23) = 15.56$, $p < .001$, $\eta_p^2 = .40$. RTs for task 1 ($m = 1044$ ms) were faster than those for task 2 ($m = 1150$ ms, see Table 1). In addition, RTs from random-order blocks ($m = 1225$ ms) were slower compared to RTs from fixed-order blocks ($m = 969$ ms), $F(1, 23) = 110.26$, $p < .001$, $\eta_p^2 = .83$, indicating the occurrence of task-order control costs.

The main effect of session was not significant, $F(1, 23) < 1$, $p = .34$, $\eta_p^2 = .04$, showing that participants had no general practice effect across the experimental sessions. However, we found a significant block type × session interaction, $F(1, 23) = 11.98$, $p = .002$, $\eta_p^2 = .34$, suggesting that the RT difference between both block types, i.e., task-order control costs, generally differed between both sessions. Subsequent comparisons revealed that task-order control costs were increased in session 2 with the sequential-order instruction ($m = 314$ ms) compared to session 1 with the free-order instruction ($m = 198$ ms), $t(23) = 3.46$, $p < .01$ (see Fig. 4). This increase in task-order control costs was specifically driven by slower RTs in random-order blocks of session 2 relative to RTs in random-order blocks of session 1, $t(23) = 2.99$, $p < .01$ (DeJong, 1995; see also Hendrich et al., 2017, submitted). On fixed-order blocks, RTs did not differ between both sessions, $t(23) = 1.51$, $p = .15$. These finding are in line with the assumption that task instructions affect monitoring-based mechanisms of task-order regulation.

We also found a significant task × block-type interaction, $F(1, 23) = 9.46$, $p = .005$, $\eta_p^2 = .29$, reflecting a reduced RT increase from task 1 to task 2 in fixed- ($m = 90$ ms) relative to random-order blocks ($m = 124$ ms), $t(48) = 3.03$, $p = .004$. Similar to Experiment 1, this suggests that, within DT trials, participants are able to prepare the switch from task 1 to task 2 when the order of tasks is known beforehand (DeJong, 1995; Liepelt et al., 2011). The interaction task × session was also significant, $F(1, 23) = 4.26$, $p = .05$, $\eta_p^2 = .16$. Post-hoc comparison revealed that the RT increase from task 1 to task 2 was larger in session 1 ($m = 117$ ms) compared to session 2 ($m = 96$ ms), $t(23) = 2.07$, $p = .05$. The observation of reduced costs for task 2 on session 2 can be explained by improved intertask coordination due to practice on the session 2 (Liepelt et al., 2011). The triple interaction task × - trial type × session was not significant ($p = .64$).

Order-switch costs

To test if order-switch costs were also modulated by instructions, we performed an ANOVA on RTs with the within-subjects factor tasks (task 1, task 2), trial type (same-order trial and different-order trial), and session (session 1 with free-order instruction and session 2 with sequential-order instruction). This analysis revealed a significant main effect of the factor task, $F(1, 23) = 18.18$, $p < .001$, $\eta_p^2 = .44$. As can be seen in Table 1, RTs for task 1 ($m = 1163$ ms) were faster than those for task 2 ($m = 1287$ ms). In addition, responses in different-order trials ($m = 1268$ ms) were slower compared to responses in same-order trials ($m = 1182$ ms), $F(1, 23) = 53.00$, $p < .001$, $\eta_p^2 = .70$, indicating the occurrence of order-switch costs. Furthermore, RTs in same- and different-order trials were faster on session 1 ($m = 1187$ ms) compared to session 2 ($m = 1264$ ms), $F(1, 23) = 8.96$ $p = .001$ $\eta_p^2 = .28$. Importantly, the interaction of trial type and instruction was not significant, $F(1, 23) = .10$, $p = .75$, $\eta_p^2 < .01$, suggesting that the RT differences between same- and different-order trials, i.e., order-switch costs, were unaffected by the instruction manipulation[2]. All other interactions were also not significant (all $ps > .08$).

[2] As in Experiment 1, we tested the posterior probability ($\Pr(H_0|D)$) of a hypothesis assuming a missing interaction of the factors Instruction group and Trial type versus a hypothesis assuming a significant interaction of these factors by calculating the Bayesian information criterion (BIC). With $\Delta BIC = 3.07$ and $\Pr(H0|D) = 0.82$ this analysis provides 'positive' evidence for the assumption that order-switch costs do not differ between the sequential- and the free-order instruction condition.

Discussion

The findings of Experiment 2 showed that (1) task-order control costs were reduced in conditions with the free-order compared to sequential-order instruction, (2) the reduction of task-order control costs was accompanied by lower RTs on random-order blocks under the free- relative to the sequential-order instruction (DeJong, 1995), and (3) order-switch costs did not differ between both instruction conditions. In addition to Experiment 1, by applying a yoked design, we demonstrated that the effect of instructions on task-order control costs occurred after controlling for different order-switch rates in both instruction conditions. Under the free-order instruction, participants switched their response order relative to the previous trial on 36.56% of trials from random-order blocks; under the sequential-order instruction, they switched their response order on 33.91% of trials, $\chi^2(1) = 1.53$, $p > .20$. Thus, the difference in task-order control costs between instruction conditions cannot be accounted for by different rates of response order switches. In addition, our results were not confounded by the applied sessionwise design of manipulating the instruction condition: RTs on random-order blocks were slower on session 2, with sequential-order instruction, compared to session 1 with free-order instruction. Thus, a potential practice effect would have counteracted against the hypothesis of increased RTs under sequential-order (session 2) compared to free-order (session 1) instruction condition.

General discussion

The aim of the present study was to disentangle the effect of instructions on task-order control and order-switch costs. For this purpose, participants performed DTs in fixed- and random-order blocks under two different instructions during random-order blocks: under the free-order instruction, participants could freely decide about the response order, while, in the sequential-order instruction, participants were instructed to respond to both tasks according to the order of stimuli. In two experiments, we demonstrated that task-order control costs, RT differences between fixed- and random-order blocks, were reduced under the free-order relative to the sequential-order instruction. Order-switch costs, the RT difference between same- and different-order trials, were unaffected by the instruction manipulation. In addition, in Experiment 2, we demonstrated that these effects cannot be accounted for by different rates of response order switches across both instruction conditions.

Task-order control costs and order-switch costs: the impact of instructions

Task-order control costs reflect monitoring-based mechanisms of task-order regulation, which can be observed when comparing DT performance in random-order compared to fixed-order blocks. According to Szameitat et al. (2002, 2006), the processing order of two tasks is regulated by a task-order control structure that represents a list of both tasks in a specific order. Performing a DT trial involves the implementation of the appropriate control structure in working memory, which then guides the processing order by sequentially activating the task sets of the component tasks. Similarly, Luria and Meiran (2003, 2006) proposed an order setting process that determines the processing order and takes place at the beginning of each DT trial. Because in fixed-order blocks, participants can employ the same scheduling strategy with the same activated order-control structure throughout the whole block, the demands on task-order regulation should be relatively low as compared to random-order blocks. In these latter blocks, the order of both tasks changes randomly from trial to trial, which causes that participants need to permanently change the task-order control structure and match it to the normative task-order specified by the order of stimuli. Therefore, to guarantee appropriate task performance, participants have to monitor the sequence of stimuli, make a decision on which stimulus was presented first, and activate the appropriate task-order control structure in random-order blocks much more frequently than in fixed-order blocks (Sigman & Dehaene, 2006; Stelzel et al., 2008; Strobach et al., 2015; Szameitat et al., 2002, 2006). As a result, RTs on random-order blocks were increased compared to RTs from fixed-order blocks, resulting in task-order control costs reflecting the occurrence of monitoring-based task-order regulation mechanisms.

Importantly, in random-order blocks under the free-order instruction, no normative task order is instructed and participants can freely decide about the response order. Consequently, participants do not have to engage monitoring processes to track the stimulus order and match the task-order control structure accordingly (DeJong, 1995). Instead, they can activate the task-order control structure based on their free order choice and can perform DTs with less reliance on monitoring-based mechanisms of task-order regulation. As we have shown in both experiments, these reduced demands on monitoring-based mechanisms result in decreased task-order control costs under a free-order compared to a sequential-order instruction.

Order-switch costs, the RT difference between same- and different-order trials, were not affected by the instruction manipulation. How can this lacking effect be explained? According to several authors (DeJong, 1995; Luria & Meiran, 2003, 2006; Schubert, 2008; Strobach et al., 2015; Szameitat et al., 2006), order-switch costs are associated with the pre-activation of task-order by an episodic memory structure of the previous DT trial: performing a DT trial results in the formation of a memory trace that contains information about the processing order and that is stored in episodic memory. This memory trace remains active over time and can influence the processing order on the next DT trial. In same-order trials, this results in a performance benefit, as the order of the previous trial is repeated, while in different-order trials, this results in impaired performance as the reactivated episodic memory structure needs to be overcome to switch the processing order of both tasks.

According to recent accounts, the ongoing activation of a task guiding memory structure seems to reflect a mechanism that inevitably (i.e., automatically) accompanies regular sensory-motor behavior. According to this understanding, the processing of any sensory-motor chain leads to an automatic storage of 'stimulus–response' event files in episodic memory, which will be activated in later episodes for guiding upcoming behavior (Hommel, 2004 see also Mayr, 2002). Most importantly, for the storage and activation of the episodic trace of task-order, it should not matter whether a certain response order is a result of a free- or a pre-determined (for the case of the sequential-order instruction condition) decision about task-order. The related order-switch costs should occur to the same degree, irrespective of whether participants have to match their task-order control structure to an externally pre-specified task order or whether they can freely decide about the processing order. In line with this assumption, we found that order-switch costs did not differ between the free- and the sequential-order instruction.

Extensions of former studies on task control

Several studies have already shown that task-order control costs (DeJong, 1995; Strobach et al., 2015; Stelzel et al. 2008; Szameitat et al., 2002) as well as order-switch costs (Luria & Meiran, 2003, 2006; Szameitat et al., 2006) are reliable characteristics of task-order control mechanisms during DT performance. However, evidence on the specific nature of these mechanisms has been scarce. In the study of DeJong (1995, see also Hendrich et al., 2017, submitted), the author could show that RTs from random-order DT blocks were lower when participants could freely decide about their response order than when they had to match the response order to the stimulus order. However, DeJong did not include fixed-order blocks nor did he investigate the effects of his order manipulation on same- and different-order trials, which makes it impossible to draw clear conclusions about how instructions influence the different mechanisms regulating and guiding task-order in DT situations.

The current study goes beyond earlier studies, because by including fixed-order blocks and by distinguishing between same-order and different-order trials, we disentangled the effect of different instructions on task-order control and order-switch costs within the same experiments and set of participants. In addition, we showed that the difference in task-order control costs cannot be explained by different rates of response order switches. In Experiment 1, we showed that when comparing random-order blocks with sequential- and free-order instructions, the rate of response order switches was reduced in the latter condition. This is in line with findings from studies with the voluntary task-switching paradigm that reported reduced frequencies of task switches (Arrington & Logan, 2005). As a result, the RT differences between the sequential- and the free-order instruction reported by DeJong (1995) as well as the differences in task-order control costs that we found in Experiment 1 could also be explained by different rates of order-switches in both instruction conditions. However, by applying a yoked design in Experiment 2, we demonstrated that task-order control costs differed between both instruction conditions, even if the frequencies of response order switches in the free- and sequential-order condition were controlled for. Thus, the difference in task-order control costs between both instructions cannot be accounted for by different-order-switch rates.

Order control in dual-task and task-switching situations

The assumption that task-order control and order-switch costs reflect distinct processes of task-order regulation is in line with evidence from task-switching studies. In the task-switching paradigm, participants perform single-task blocks, in which one task is repeated, and mixed-task blocks, in which one of two tasks is presented per trial and participants have to occasionally switch tasks (Kiesel et al., 2010). Similar to the current approach, two different types of costs can be distinguished. On a trial level, within mixed blocks, switching costs reflect the difference between task repetition and task switch trials (Rogers & Monsell, 1995). On a block level, mixing-costs reflect RT differences between single- and mixed-task blocks (Koch, Prinz, & Allport, 2005; Rubin & Meiran, 2005). In recent years, it has been suggested that mixing and switching costs represent distinct mechanisms of task control. Evidence for this assumption comes from studies showing that both types of costs can be dissociated on a behavioral level (e.g., Kray & Lindenberger, 2000) as well as from studies that found different neural correlates for both types of costs (Braver, Reynolds, & Donaldson, 2003). From this line of research, it has been assumed that switching costs reflect rather transient processes that are exclusively relevant for shifting from one task to another (Braver et al., 2003). Mixing-costs, on the other hand, seem to reflect rather sustained components of cognitive control that ensure flexible switching between tasks, such as engaging attentional monitoring processes that are sensitive to information signaling task changes (Koch et al., 2005; Rubin & Meiran, 2005). Such sustained control processes would be equivalent to the monitoring-based mechanisms necessary to regulate the processing order of two tasks in random-order DT blocks. In mixed blocks as well as random-order blocks, participants have to maintain different task sets and task-order control structures, respectively. In addition, they have to collect information on which task or which task-order to execute and employ attentional processes accordingly. Thus, mixing-costs and task-order control costs may reflect similar global and sustained control processes that are necessary to flexibly adapt to changing task demands in multitasking situations.

Conclusion

To conclude, we investigated the effect of instructions on additional mechanisms that arise in DT situations with varying task-order. We demonstrated that task-order control costs were reduced under a free-order compared to a sequential-order instruction. This type of costs that occurs on a block level seems to reflect global monitoring-based mechanisms of task-order regulation, such as employing monitoring processes and activating an appropriate task-order control structure in working memory. Contrarily, order-switch costs, that occur on a trial level and reflect memory-based mechanisms of task-order guidance, were not affected by the instruction manipulation. Based on this dissociation, we conclude that both types of costs reflect distinct mechanisms that regulate and guide the processing order of two tasks in DT situations.

Compliance with ethical standards

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Ethical approval All procedures performed in studies involving human participants were in accordance with the ethical standards of the institutional and/or national research committee and with the 1964 Helsinki declaration and its later amendments or comparable ethical standards.

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Eidesstattliche Erklärung

Hiermit erkläre ich an Eides statt,

1. dass ich die Dissertation selbstständig und nur unter Verwendung der angegebenen Hilfen und Hilfsmittel angefertigt habe,

2. dass ich mich anderwärts nicht um einen Doktorgrad beworben habe und dass ich keinen entsprechenden Doktorgrad besitze,

3. dass ich die Dissertation oder Teile davon nicht bereits bei einer anderen wissenschaftlichen Einrichtung eingereicht habe und dass sie dort weder angenommen noch abgelehnt wurde,

4. dass ich die der dem Verfahren zugrunde liegenden Promotionsordnung der Lebenswissenschaftlichen Fakultät der Humboldt-Universität zu Berlin vom 5. März 2015 zur Kenntnis genommen habe und

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Sebastian Kübler